the Skeptical Inquirer

Remote Viewing Revisited

Two Tests of Diviners / Planets and Radio Waves
Great Lakes Pseudomystery
The 'Closed Minds' Gambit

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the Skeptical Inquirer

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COVER: "Cloud Mirror" from the original painting by Thomas Akawie ©1975.

News and Comment

Everybody Loves a Doomsday (Prediction)

The dreaded doomsday date of March 10, 1982, has come and gone, and at last glance California was still attached firmly to the rest of North America. The rest of the world, for better or worse, continues to go on as well.

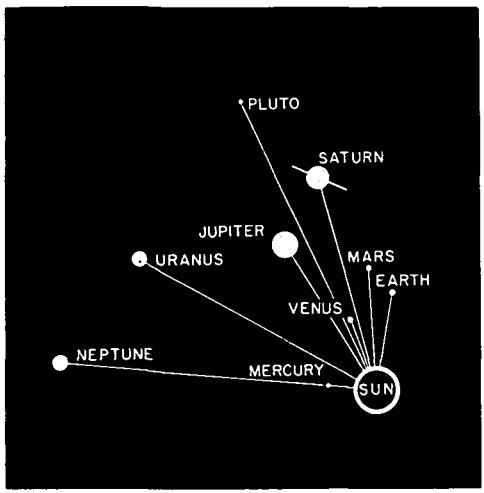
Everybody loves a doomsday—or at least an excuse for a doomsday prediction—and the planetary "alignment" of March 1982 was as good an excuse as any to conjure up dramatic images of the end of the world. In fact, it was probably the best we will have until the year 2000—the millennium, the ultimate Super Bowl of doomsday delights.

March 10, 1982, really didn't have all that much going for it, as doomsdays go. If it hadn't been for John Gribbin and his book *The Jupiter Effect*, we'd probably have let it all pass with no more thought than we give to, say, February 11, 1979. And who remembers anything that happened that day?

But March 10, 1982, as any concerned occultist (or anyone who had talked to one) would tell you, was the day all the planets would line up, and everyone knew what that meant: disaster. (It's not for nothing that the very word "disaster" has an astrological origin, "bad star.")

Never mind that each of the geophysical links in the chain Gribbin and co-author Stephen Plagemann had proposed for the Jupiter effect was weak to nil. Never mind that the whole idea had been almost universally discounted by the scientific community. Never mind that Gribbin himself had become concerned enough about the ludicrous distortions of the occultists to publish a retraction of the theory and a *mea culpa* (June 1980 *Omni*, summarized in the Fall 1980 SKEPTICAL INQUIRER). For that matter, never mind that the planets wouldn't even be lined up in 1982 (they would occupy slightly more than one quadrant, 95 degrees of arc). Never mind that, as mathematical astronomer Jean Meeus pointed out (*Sky and Telescope*, January 1982), the eight major planets have occupied the same 90° sector of the solar system on 18 occasions since A.D. 1 (and this wasn't one of them) and will do so seven more times before the year 3000. (The next, alas, is not until 2161.) Never mind that on April 11, 1128, the eight major planets occupied a sector of the sky only 40° across and that in early 1962 we managed through a spectacular alignment with four planets arrayed against four others on the opposite side of the sun, all without untoward effect. Never mind all that. We need our doomsday predictions, and we don't

Scientists: 'LA will disappear Wednesday'



Directions of the planets from the sun on March 10, 1982. (Sizes and distances not to scale).
Kitt Peak National Observatory

want anybody to take away our excuses.

Astronomers and observatories had more than their usual share of inquiries from citizens worried about the coming crumbling of California (or worse), and they gamely sought to allay unfounded fears. The *Griffith Observer*, published by the Griffith Observatory in Los Angeles, devoted most of its January 1982 issue to debunking the Jupiter effect, and the observatory prepared a planetarium show devoted to it. The Smithsonian Astrophysical Observatory in Cambridge, Mass., had put together a packet of information years ago and had it at the ready. And, in February, Kitt Peak National Observatory in Tucson distributed to the news media an eight-page "backgrounder" clearly taking up each of the six links in the Jupiter-effect theory and pointing up their inconsequentiality. "People across the country are writing the Public Information Office and expressing concern about what has been billed as 'the great planetary lineup' or 'superconjunction' of 1982," began the Kitt Peak release. "Many express their concern over rumors they have come upon that this alleged lineup will cause devastating earthquakes and perhaps the end of the world. We wish to provide some relevant information." The packet stimulated some responsible reporting from the media to counter some of the more sensational coverage.

Anyway, March 10 passed uneventfully, and by April 10 the planets were no longer even on the same side of the sun.

Doomsday didn't happen. But don't be disappointed, doomsday fans. A.D. 2000 is only 17½ years away.

—Kendrick Frazier

Lourdes Revisited

The 1982 *Encyclopaedia Britannica Medicine and Health Yearbook* has taken note of the current interest in the paranormal by including a 19-page, fully illustrated article by Ellen Bernstein on the alleged miraculous events said by the faithful to occur at the third-largest tourist attraction in France—Lourdes. The shrine's 400 hotels continue to be jammed every year by throngs of the afflicted who hope that the Blessed Virgin will appear to them and bring them cures.

Though countless undocumented recoveries are related by pilgrims, and niagaras of Lourdes water are sold in tiny vials to the hopeful around the world, the record of this important center of religious faith-in-action lacks conviction, concludes the *Britannica* account.

When Bernadette Soubirious, in 1858, reported a series of visions at Lourdes, there was no claim or intention to portray the area as a center of healing. Nor was the water from the local spring touted as exceptional or curative; indeed, analysis shows it to be lacking in any special mineral or trace-element. It is simply plain spring water.

It is significant that of the vast armies of pilgrims who pass through Lourdes—almost five million in 1979 alone—only 64 "cures" have been accepted by the church, though thousands more have been endorsed by less official sources and medical authorities; and, since the improvement in medical understanding and research that has occurred from the early days of Lourdes to the present, the number of claimed cures has dropped from a yearly average of 75 to less than one.

The latest "inexplicable" certified cure to be declared by the international committee, in 1976—subsequently accepted by the Roman Catholic Church in 1978—has been examined by U.S. doctors and described as "vague" and "obtuse" by them. The documents were classified as "a lot of mumbo-jumbo" and as "unscientific and totally unconvincing." Essential lab tests were never performed, and the symptoms displayed by the patient were typical of hysteria—a condition very likely to respond to suggestion and attention.

The *Britannica* article points out that the very special attention and love that the ailing receive at Lourdes cannot be discounted and probably account for the impressive display of discarded crutches and other aids that are shown at the shrine. Medical science now understands much better the impact of such psychological effects in aiding recovery and activating the body's immune systems.

As Bernstein concludes: "Lourdes has never swindled the sick with false promises. Nonetheless, a better effort could be made to convey to the world what the shrine really has to offer. Lourdes is *not* a miracle mill; it is *not* a spa; and the water does *not* have healing powers. Lourdes *is* a unique international meeting place, and there is every reason to believe that the sick, the frail, and other sufferers derive innumerable psychological and spiritual benefits from their visits. Wonders do occur when pilgrims discover their own strengths and learn the art of living with their maladies."

Perhaps this modest set of facts casts doubt upon the supernatural claims made in behalf of Lourdes: Bernadette herself suffered all her life from severe asthma, and developed tuberculosis after her visitation by the Virgin. She was bedridden the last years of her life and expired at an early age of the many complications of her illnesses.

—James Randi

Lens Flares as UFOs

Many reported UFOs can be shown to be neither in the sky nor in the mind of the reporter, but rather within the optical system of a camera. There are a great number of photographs that show mysterious lights and shapes not seen by the photographer at the time of the shutter release, and these are gleefully snapped up by believers as evidence of UFOs. They are hardly that, since such phenomena are known as lens flares and are easily recognized and proven to be such.

Any optical system has imperfections that result in strange aberrations. A common lens-flare occurs in a typical camera when a very bright source of light is overexposed. It may well be lost in the brightness of a surrounding background but will show up prominently if that background is dark. Modern coated lenses, used in even the cheapest cameras today, minimize this problem—but a lens flare *will* show up in the best of cameras if the originating source is bright enough.

Flares are the results of internal reflections within the lens. One can safely assume that the center of any uncropped film-frame is also the center of the lens, optically, and coincides with the optical axis. To examine a photo for flares, draw a line from any "UFO" image through the center of the photo. It should extend an equal distance beyond the optical center to a bright light source in the photo and will be an inverted image of the bright source.

Such is the case with a "UFO Update" article appearing in the December 1980 *Omni*. The fountains of Long Lake, near Silkeborg, Denmark, provided the bright sources, and against the dark sky appeared a pair of UFOs. Though the *Omni* reproduction failed to show it, the "UFO" image was quite startling, appearing as an inverted saucer with rays of light pouring down. This was an inverted image of the fountain structure itself. The test for lens flare outlined above checked out quite well on this photo.

The comments in the article concerning faking of the photo were beside the point. Certainly the photographer neither *faked* anything nor *saw* anything. Months had passed before the film was developed and this presumptuous conclusion of UFOs was accepted.

In an earlier issue of *Omni* (April 1979), a photo appeared of an alleged string of UFOs over Washington's Capitol dome. These, too, responded to the flare test well, and each "UFO" corresponded to a street light in the foreground. The investigator for *Omni* now admits the "UFOs" in the Capitol dome photo are the result of lens flares, but why did he accept the Silkeborg photos, which resulted from exactly the same thing?

Other basic questions arise about the Silkeborg photo. Why were there no reports from the city itself, over which a "30 metre"-wide UFO supposedly hovered? The "extensive photo analysis" claimed for this photograph evidently did not include an examination for simple lens-flares. Why not? How did the computer analysis tell investigators that the object was "800 meters from the camera" when it was *inside* the camera?

Other strange things can happen inside a camera. Many an amateur has enthusiastically exhibited a spurious UFO photo showing the famous "spindle" shape. This looks like the result of glueing together the two bases and stems of a pair of champagne glasses, and is another form of flare that arises from strong light sources reflecting from lens surfaces but not resolving the original image. A line drawn through

the long axis of the spindle shape will intersect the source and pass through the center of the photo. Usually, several "UFOs" are found strewn about the photo, all pointing at the source.

Any competent photographer can provide a further assortment of possibilities for flare images, some showing up in photos without any trace of an original source, which can be just out of the frame. These can be prevented through the use of lens hoods, which were developed to solve this problem.

Experiment for yourself. Open your camera lens wide and snap a photo of a street light at night. Keep the source off-center and expect interesting flares. But don't start yelling that the Martians are coming.

It would behoove the UFO investigators, rather than applying advanced technology to these reports, to use simple common sense and high school science to eliminate at least these obvious bits of fluff that cloud the entire controversy over UFO reports.

—James Randi

Conviction Prediction

The year 1981 was not a good year for Australian clairvoyant Tom Wards. Wards bills himself as "an international clairvoyant" and places classified ads claiming phenomenal accuracy. Not so this past year.

In June he predicted that the Pope would die "in a month or so." The month expired but the Pope didn't.

In August he had a try at the possible winner of the local Australian Rules Football final. He predicted the winner would be a team called Collingwood. When they lost, his regular advertisment in a daily paper changed to the third person and said, "It is unfortunate that Mr. Wards's weakness is in football predicting, and with sheer motivation I am sure Mr. Wards's strength held to take Collingwood into the finals, but they seemed to lack the stamina of the psychic."

Wards's predictions on the budget were wrong, including the one that a cabinet minister would resign in protest over the budget. Six months later the cabinet minister had still not resigned.

Wards, however, did feature in an Australian government publication—the Annual Report of the Taxation Commissioner. Wards was accused of understating his taxable income by $37,294 between 1976-77 and 1977-78. He was requested to pay additional taxes of $11,410 and a penalty tax of $3,575. Tom Wards said he was not surprised when taxmen told him of his monetary neglect: "I knew about it and my prediction was backed up by a friend who is an attorney and who is also psychic"!

—Mark Plummer

Mark Plummer is co-chairman of the Australian Skeptics.

A Welcome and a Request

A belated welcome to the more than 2,000 new subscribers who have joined the ranks of SKEPTICAL INQUIRER readers since the beginning of the year. This is the largest one-time jump in circulation since our inception five and a half years ago. Most of you new readers learned about us from Douglas R. Hofstadter's laudatory article in the February *Scientific American*. We hope you will find our approach to paranormal and fringe-science claims a refreshing change from the credulous treatment typical of proponents and most other publications. The SKEPTICAL INQUIRER has been trying to

pioneer the critical examination of such claims from a tough-minded, skeptical, scientific, and responsible point of view.

More important than our steady growth is the high quality of our readership. Our journal is read by important educators and opinion leaders worldwide, and so our influence is large in proportion to our numbers.

Those of you who have obtained back issues and are familiar with our previous discussions know that my colleagues on the editorial board, our consulting editors, and I encourage the participation of SKEPTICAL INQUIRER readers in this process of inquiry and evaluation. Send us your ideas, suggestions, and criticisms. Help us keep track of fringe-science goings-on in your area. Let us know of worthy critiques published in your local media that deserve praise, of reports that deserve criticism, of new subjects that need evaluation.

There are other things you can do to help. Tell your friends about the SKEPTICAL INQUIRER and our effort to cast the light of reason and critical inquiry on fringe-science claims. Tell your colleagues, your libraries, the editors of your local newspapers, and the news directors of your local broadcasting stations. Help us keep them informed of the *real* facts behind sensational and exotic claims of the paranormal instead of the unexamined assertions and anecdotal stories that too often pass for the facts.

Join us in our task of presenting reliable information about claims on the paranormal borderlands of science and, where the facts aren't yet known, help us to go after them. We think we can all enjoy the quest, and do some good for public education at the same time.

—*Kendrick Frazier*

UFOs and the Tabloids: The Education of a Writer

The front page of the March 2, 1982, national supermarket tabloid *Globe* announced, "UFOs: At Last the Proof." The article inside was headlined "UFOs Are Watching" and "Proof at last of alien spies." Another headline said, "Cases the skeptics can never dismiss."

"There is finally scientific proof that visitors from outer space are studying us, claims a top UFO expert," the article began.

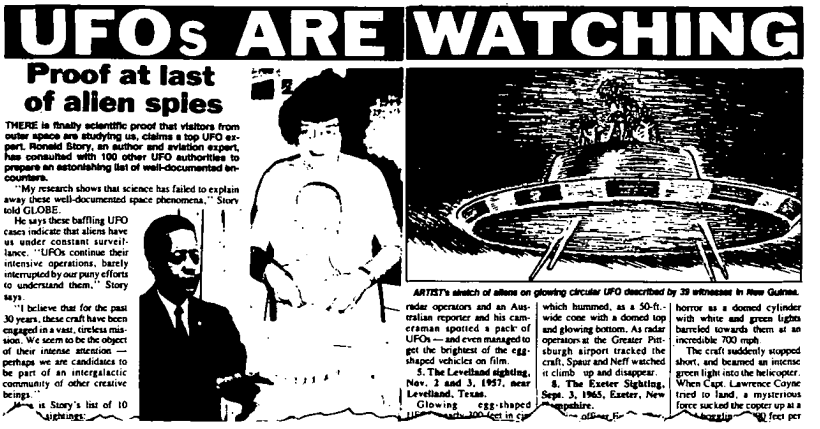
It referred to the expert's "astonishing list of well-documented encounters" based on consultations with 100 other UFO authorities.

"These baffling UFO cases indicate that aliens have us under constant surveillance. 'UFOs continue their intensive operations, barely interrupted by our puny efforts to understand them,' " the article reported, quoting the expert.

Before beginning a synopsis of the "10 key UFO sightings," it quoted the expert again in dramatic fashion: "I believe that for the past 30 years these craft have been engaged in a vast, tireless mission. We seem to be the object of their intense attention—perhaps we are candidates to be part of an intergalactic community of other creative beings."

The only trouble is that the man these dramatic statements are attributed to says he never said them. The remarks are credited to Ronald D. Story, author of the 1981 book *UFOs and the Limits of Science*. Story, you may recall, is the same person who called our attention to *Fate* magazine's selective deletions of comments critical of UFO proponents and favorable to skeptical UFO investigators from a review he wrote for *Fate* (see *SI*, Winter 1981-82).

That, Story feels, was bad enough.



UFOs ARE WATCHING

Proof at last of alien spies

THERE is finally scientific proof that visitors from outer space are studying us, claims a top UFO expert. Ronald Story, an author and aviation expert, has consulted with 100 other UFO authorities to prepare an astonishing list of well-documented encounters.

"My research shows that science has failed to explain away these well-documented space phenomena," Story told GLOBE.

He says these baffling UFO cases indicate that aliens have us under constant surveillance. "UFOs continue their intensive operations, barely interrupted by our puny efforts to understand them," Story says.

"I believe that for the past 30 years, these craft have been engaged in a vast, tireless mission. We seem to be the object of their intense attention — perhaps we are candidates to be part of an intergalactic community of other creative beings."

Here is Story's list of 10 sightings:

ARTIST's sketch of aliens on glowing circular UFO described by 39 witnesses in New Guinea.

rader operators and an Australian reporter and his cameraman spotted a pack of UFOs — and even managed to get the brightest of the egg-shaped vehicles on film.

5. The Levelland sighting, Nov. 2 and 3, 1957, near Levelland, Texas.

Glowing egg-shaped [illegible] each 300 feet in [illegible]

which hummed, as a 50-ft.-wide cone with a domed top and glowing bottom. As radar operators at the Greater Pittsburgh airport tracked the craft, Spaur and Neff watched it climb up and disappear.

8. The Exeter Sighting, Sept. 3, 1965, Exeter, New Hampshire.

[illegible] officer Fr[illegible]

horror as a domed cylinder with white and green lights barreled towards them at an incredible 700 mph.

The craft suddenly stopped short, and beamed an intense green light into the helicopter. When Capt. Lawrence Coyne tried to land, a mysterious force sucked the copter up at a [illegible] 60 feet per

Portion of Globe article "quoting" Story.

But the *Globe,* he told the SKEPTICAL INQUIRER, resorted to total fabrication.

"I did speak to a reporter from the *Globe,* about a year ago, but said nothing more pro-UFO than that there are a few unexplained cases (not 'space' phenomena) of UFOs that keep the question open as a genuine mystery."

He heard nothing more from the *Globe.* Then the March 2 issue appeared. "I must admit I was completely stunned," says Story. "What amazes me so much about the *Globe* article is that it is a total fabrication, not a distortion or misquote—I said nothing of the kind. It is a complete fantasy and not a single word of it is mine!

"Just when you think that certain elements of the media are content with a little twisting of the truth by misquoting or quoting out of context, you find now that nothing short of total invention will do."

Since publication of his book, Story is learning some hard lessons about the extent to which pro-UFO believers and publications will go to mislead. The book dealt with ten top UFO cases. While not clearly pro-UFO, it was not consistently skeptical or critical of purported evidence either (Story says he has become more skeptical since then). This, apparently, made pro-UFO periodicals feel he was in "their camp" and that he wouldn't object to their using his name and work for their own purposes.

For instance, the *Weekly World News,* another tabloid, published a five-part serialization of his book, in its Nov. 10 through Dec. 1, 1981, issues. Story says he agreed that the editors could do some minor editing to fit it to the serialization format. He wasn't prepared for what happened. The editors completely rewrote the five chapters they used.

"Compare these [the five installments in *Weekly World News*] with my book and notice the differences," says Story. "Not one sentence reads the same." He's right. Although some *phrases* are intact, each of the chapters has been totally restructured and rewritten to make the incidents seem more dramatic and mystifying than Story had presented them. A spot check showed no sentence, except for some quotations of other persons, identical to what Story wrote in his book. Story calls the distortions created by the tabloid's rewrite "outrageous."

—Kendrick Frazier

Superstition and the Press

* * * * *

Curtis D. MacDougall, a respected figure in journalism education in the United States for well over half a century (his classic textbook, *Interpretative Reporting,* has just come out in a Golden Anniversary edition), is hard at work on a sequel to his book *Hoaxes.*

Hoaxes, first published in 1940 but still in print (Dover), dealt with all manner of fraud artists. MacDougall says his new book will focus specifically on "the role of the press in perpetuating ignorance and superstition." Unfortunately, he has found no shortage of material. "I'm overwhelmed by the amount of material at hand. It's fantastic the way the material keeps pouring in... I'll be rewriting chapters as long as possible." The book's chapters will run the gamut from astrology to witchcraft, examining the press's role in almost every occult and fringe-science topic.

"The facts speak for themselves," says MacDougall, professor emeritus of journalism at Northwestern University, now working full-time on the book at his home in Evanston. "I spell out what [CSICOP chairman Paul] Kurtz keeps saying about the one-sidedness of the press. I really don't have to do much editorializing; the evidence speaks for itself." The unwritten code of the news media in dealing with the paranormal and fringe-science is to go for entertainment, regardless of the truth, says MacDougall. "The rule is, play it for its immediate sensational value. As a result, the majority of stories have no follow-ups." Thus few ever learn the actual facts of an incident, even once they do become known.

"This will be my twenty-sixth book manuscript," says MacDougall. "Most of the rest have been meaty, but they have a beginning and an end. The superstition book just goes on and on."

There seems little doubt that even responsible newspapers have a double standard regarding such matters. Political and economic news is treated seriously and professionally. Stories about the paranormal, especially "psychics," are judged primarily according to their entertainment value, without much concern about checking the assertions of the "psychics" being quoted.

"I suspect the reason stories about phony psychics get in the papers is that, even though editors know better, they think such stories make good copy," says Spencer Klaw, editor of the *Columbia Journalism Review.*

"Reporters treat such stories as features," says Melvin Mencher, professor at the Columbia University Graduate School of Journalism, author of *News Reporting and Writing,* and investigative-reporting teacher to a generation of journalists, "and we all know since Janet Cooke that truth is irrelevant as a criterion for such pieces." Mencher says he's pessimistic about the chances of ever changing that situation. "Intellectuals will continue to read and ponder; but the media are aimed at the mass of readers and listeners who love to be entertained by stories like those about psychics and their folderol."

—*K.F.*

Don't You Remember These?

At the beginning of 1981, the *National Examiner* proudly gave us the predictions of a Columbia, S.C., "psychic" named Mary Green. For $75,000 a year, she predicts the future for her clients, and she made these predictions for 1981:

1. The Ayatollah Khomeini will

be pushed off the top of a mosque in Mecca.

2. Interest rates will reach 25 to 30 percent.

3. Washington State and New England will be ravaged by floods, tidal waves, and earthquakes.

4. Five supposedly dead volcanoes will come to life. (Where they are located was not mentioned.)

5. There will be "an unbelievable cataclysm of nature" after which hundreds of wounded will be healed with "crystal."

6. Russian troops will enter Israel.

7. There will be "a heated dispute over Iran."

8. "A double-cross of Egypt will shake the world."

9. The U.S. draft will return.

10. There will be a conflict "similar to Vietnam or Korea."

11. The ERA will become law.

12. China will detonate a nuclear device at the beginning of summer.

13. Japan and the Philippines will be covered by "enormous clouds of radioactivity."

Not so good, Mary.

—*J. R.*

Love, Tony

An episode of Tony Randall's TV series, "Love, Sidney," last December was refreshingly blunt about paranormal matters. Randall, who is known to look askance at psi claims, used the character he plays to call telepathy "guessing" and label a spirit seance as "trash," "lies," "sham," "fraud," and "imitation." He was asked by the little girl on the show, "What's 'parapsychology'?" and he replied, "Call it by it's generic name, 'hokum'!"

During the seance to call up his departed mother, Sidney cast his eyes to the ceiling and pleaded, "Call after eight. It's cheaper!"

—*J. R.*

A Record Performance

Arthur Lintgen, a physician from Pennsylvania, recently demonstrated a most unusual ability: He can identify the music on a phonographic record simply by examining the pattern of grooves on its surface! Yes, folks, he really can!

Called by *Discover* magazine to look into this claim, I assembled a number of recordings, carefully covered the labels and matrix numbers, and hurried off to visit this man, very much doubting that he could perform as advertised but willing to be shown. The demonstration was mind-boggling.

Dr. Lintgen simply glanced for a moment at each of the numbered recordings and gave his opinion. If the target was not a record of a fully orchestrated composition, classical, post-Mozart, he declared it out of his field. A pair of Mozart symphonies were thus guessed as either Schubert or Mozart, but were not counted except as "control" items. Lintgen spotted two different recordings of Stravinsky's "Rite of Spring" and declared one control to be "gibberish" and "disorganized." That one turned out to be an Alice Cooper number.

He correctly identified, without qualifications, Ravel's "Bolero," Tchaikovsky's "1812 Overture," Holst's "The Planets," and Beethoven's Sixth Symphony—though he expressed puzzlement over an additional cut on the disc, finally identifying it as the "Prometheus Overture." One control amused him. "This is not instrumental music at all" he declared, squinting at it closely. "I'd

guess that it's a vocal solo of some kind." It turned out ot be a record of a man speaking, titled, "So You Want to Be a Magician." Certainly, Arthur Lintgen comes as close to that definition as I ever hope to see!

—*James Randi*

Group in Quest of "Unexpected" Animals

A group of biologists and others interested in claims of anomalous animals (Bigfoots and Loch Ness creatures are only the most obvious examples) have formed the International Society of Cryptozoology to serve as a professional focal point for investigation of such matters.

The organizers consist primarily of scientists who tend to believe there may be something to frequent reports of still-to-be-documented unusual creatures, and they claim their goal is to professionalize the inquiry and upgrade the level of discussion and debate. States their official prospectus: "The Society recognizes that much emotional debate has resulted over the possible existence of such animals, and that future work in this area will not be without controversy. However, the Society will attempt to address these issues in a strictly objective manner, following established scientific methods."

The new group defines the object of their interest as matters related "to animals of unexpected form or size, or unexpected occurrence in time or space." The group intends to "encourage the scientific examination of all evidence related to these matters."

J. Richard Greenwell, secretary of the Society, says "unexpected" is the key word. Examples of once "unexpected" animals that were eventually confirmed are the gorilla (1855); the okapi, a Miocene giraffid (1902); the pygmy hippopotamus (1913); the coelacanth fish (1938); and Megamouth, a new species, genus, and family of shark found in 1976.

The Society was established at a meeting at the Smithsonian Institution in Washington on January 8-9, 1982. Besides Greenwell, the officers are Bernard Heuvelmans (president) of the Center for Cryptozoology in France and Roy P. Mackal (vice president), the University of Chicago biologist long interested in anomalous-animal claims and leader of a recent expedition into the Congo in search of evidence of still-living dinosaur-type creatures. On the board of directors are George R. Zug, Chairman of the Department of Vertebrate Zoology at the Smithsonian's National Museum of Natural History, and eleven other life scientists from seven countries.

Further information about the Society, its journal, *Cryptozoology*, and its newsletter can be obtained from Greenwell. Write: Secretary, International Society of Cryptozoology, P.O. Box 43070, Tucson, AZ 85733.

—*K.F.*

Judge Rebuts Tut Suit

A San Francisco judge ruled in February that a police officer who suffered a stroke while guarding the King Tut exhibit in that city in September 1979 was not entitled to a requested $18,000 disability payment for his claim that he was stricken by the curse of King Tut.

San Francisco Superior Court Judge Richard P. Figone ruled on February 9 that the court did not need to address the "so-called mythological curse of King Tut."

"The spectators who attended the exhibit may just as well have 'disturbed' the remains of the deceased," Figone wrote. "Officer LeBrash, if anything, prevented desecration of these remains." Officer George LaBrash had claimed that the spirit of King Tut had lashed out at him for disturbing the dead, and that his stroke was a job-related injury.

Neither LaBrash's petition nor Judge Figone's decision mentioned that, according to a report in 1980 by Richard Adamson, a security officer who slept in the tomb after it was breached in 1922, the story of a curse was cooked up by expedition leaders Howard Carter and Lord Carnarvon to frighten off anyone who had thoughts of robbing the tomb at night (*SI*, Fall 1980).

—*K.F.*

Low-proof Arguments

"Intellectual ferment" can yield odd products. A recent Massachusetts college debate on the resolution that "the earth is flat," for example, presented the argument for the affirmative that people had lost their effervescence and, like champagne, could therefore be called "flat." There is quite a head (or foamy concentration of bubbles) on many an idea, however; many of our bubbles do not seem to have burst. Among the ideas with low proof but high intoxicating ability are the claims of Barry Fell to have found "ancient inscriptions" all over America that prove that virtually *everyone* beat Columbus to the New World. In the *MENSA Bulletin*, Tertius Chandler calls Fell's books "blockbusters" and praises them to the heavens. In the February 1982 *MENSA DIGSIG* (a MENSA Special Interest Group newsletter he edits) he discusses "The Rise and Fall of Tiahuanaco" and uses arguments that might make Barry Fell blanch: The founder of Mayan Chichen Itza was an English Saxon; Mexico's Quetzalcoatl was from Iceland (Björn, son of Asbrand) and a "hard man" who set in motion vast wars that forced Mayans south where they founded the Inca empire in Ecuador and the Chimu kingdom further south in Peru, which brought about the demise of Bolivia's Tiahuanaco. And if you believe this, you will believe that high I.Q.'s equate with good sense!

—*John R. Cole*

John Cole is an anthropologist at the University of Northern Iowa.

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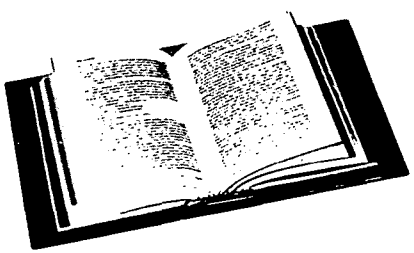

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Psychic Vibrations

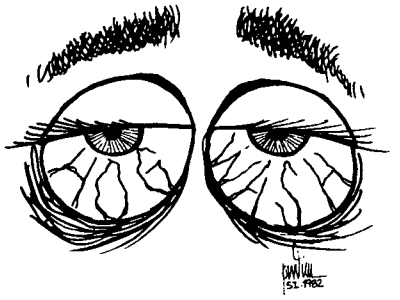
Marcel Vogel, who always describes himself as "senior scientist at IBM for over 25 years," is a chronic lecturer on "paranormal" subjects. His investigations have covered such diverse topics as Kirlian photography, the "Backster effect" (telepathic plants), and spoon bending, and he was one of the few to take seriously the "UFOs from the Pleiades" hoax.

Now Vogel is in the forefront of the newest California occult fad—healing with the "energy" of quartz crystals. Vogel recently gave a dramatic lecture-demonstration of crystal power to the Holistic Health Center of San Jose. He began by telling of his "full-blown out-of-body experience" on a mountain in India under the guidance of a Swami. (Afterward, said he, he had so much energy that water nearly boiled when it contacted his skin.) He went on to explain how each crystal has to be customized to the user's "vibrational rate" for maximum effect. Vogel then demonstrated for the audience the amazing strengthening power of the crystal. Two subjects were told to hold out their right arm parallel to the floor. He then attempted to pull down each subject's arm, which he did easily. But with the crystal held to the subject's "witness bone" (a newly discovered anatomical feature in the vicinity of the sternum), Vogel professed to be unable to pull down the arm of either subject. This was a sham, because any healthy adult can easily pull down anyone's arm. Try it yourself. (This is a favorite trick among occultists and medical quacks to demonstrate the "strengthening" effects of their hokum.)

A third subject was skeptical of the crystal, suggesting a placebo effect. Vogel did the experiment a third time, using the skeptic as his subject. With an empty wine bottle held over the "witness bone," Vogel pulled the arm down easily. Then replacing wine bottle with crystal, Vogel once again pretended to be unable to pull the subject's arm down. But the third subject was more perceptive than the first two. "It feels like you're not pushing on my arm with the same force," he said at the close of the experiment.

* * * * *

As if Healing Crystals were not advanced enough, the Holistic Health Center is now offering instruction in Sclerology, the study of the red lines in the whites of the eyes. Diagnosing a person's state of health using the iris, called iridology, is a well-established occult practice, but the use of the sclera represents a significant breakthrough in medical diagnosis. According to the Center, "The American Indians have



observed the sclera for over 1,000 years. It's an age-old technique of reading the red lines in the sclera (whites of the eyes). Used in conjunction with dermaglyphics, the reading of the lines, callouses, and markings on the bottom of the feet, through techniques developed by Dr. Stuart Wheelwright, these readings can reveal the health condition of the entire body."

* * * * *

When the Soviet chess champion Anatoly Karpov defeated Victor Korchnoi for the second time last November to win the world chess championship, one might understandably conclude that Karpov had proved himself to be the unchallenged master of the game. But that's not the way Korchnoi sees it. Two days after losing the match 6 games to 2, Korchnoi charged that he lost not to superior skill but to "strange forces." "I understand that big groups of people are developing parapsychology, in America by the CIA and in the Soviet Union by the KGB," Korchnoi was quoted as saying in the *St. Petersburg Times*. "I suspect I have been a victim of strange forces." Sounds more like sour vodka.

* * * * *

In a recent "close encounter of the third kind" in Bavaria, a witness undergoing hypnotic regression described the UFO creatures as having the same structural features as Kermit the Frog, star of Sesame Street and the Muppet Show. For some reason the investigators concluded that the incident was "hallucinatory." But let us not be hasty—"there are more things in heaven and earth . . . than are dreamt of in your philosophy." Perhaps genuine UFO aliens *do* look like Kermit the Frog.

* * * * *

The well-known British UFO researcher Gordon Creighton, frequent contributor to the "respected" *Flying Saucer Review*, still is laboring under the delusion that the Petrozavodsk "jellyfish UFO" of 1977 is a deep mystery. That "UFO" was explained almost immediately by researcher James E. Oberg as being the launch of the Soviet spy satellite Cosmos 955 from Plesetsk, only 200 miles from Petrozavodsk. This explanation was published in the Fall-Winter 1977 issue of *SI*, in *Science News*, and in various UFO journals. Yet in the June 1981 issue of *Flying Saucer Review*, the world's "leading" publication on UFOs, Creighton writes that "there seems to be no doubt that what happened at Petrozavodsk was something most impressive. Very likely we have not yet heard of its more remarkable features."

* * * * *

Omni magazine reports a significant new development in the study of the Shroud of Turin. Long Island accountant Michael Smith, who describes himself as a devout Catholic, claims that the image visible on the shroud is not that of Christ, but of none other than Tutankhamen, Pharaoh of ancient Egypt. According to Smith, the shroud and the mummy are identical in height

and structure and show many similar features.

* * * * *

An article in the February 1982 issue of *Fate* magazine, that scourge of skeptics everywhere, reports in all apparent seriousness that the grave of one George Smith in Arapho, Oklahoma, periodically bellows out "Robina [Smith's daughter] has not been saved!" One witness reports that "when I'm out in the fields, I hear ol' George bellow out every week or so."

* * * * *

That same issue of *Fate* also reports (next to an ad proclaiming "The Space People Are Coming!") that a "Holistic Healing" seminar will be transmitted across America via a satellite broadcast, to "collect and disseminate information relating to New Age topics." The cost to viewers is a mere $75. How ironic that space-age technology is now being called upon to promote stone-age beliefs.

* * * * *

Readers taking an interest in the many reports of "cattle mutilations" (often attributed to UFOs) would be interested in the November 1981 issue of *St. Louis* magazine, with its account of the cattle mutilation hysteria in Elsberry, Missouri. Author Joe Popper lays much of the blame for the widespread misinformation on the local news media, especially Channel 2 in St. Louis. "On the day the TV crew arrived, the sum total of 'weird' events amounted to a few dead cows that, in the eyes of several farmers and the local police chief, appeared mutilated. A respected veterinarian did not share their opinion." But after the TV news reports had been aired "in a hyped-up and sensationalized fashion," "carloads of people, drawn by the TV stories, were arriving in Elsberry every night." Night after night, Popper reports, "Channel 2 hammered away at the story, leading with it, pushing it, milking it." A number of dead flies were found clinging to branches, and a University of Missouri entomologist attributed their deaths to a fungus infection that afflicts insects. But Channel 2 reported that the flies died because they had "apparently fed on dead cows," and that the insects were "welded" to branches by some mysterious force. Reports of UFO sightings soon began to increase. Veterinarian William Newberry, who examined carcasses of the animals, said, "I understand why the farmers thought that there had been a mutilation, because in the state of decomposition the cow was in you could rationalize that her hide had been cut by a knife. But the mutilations were actually caused by small predators eating at the carcass." But nobody wanted to hear this, and the sensationalized accounts of "cattle mutilations" and UFO sightings continued, the flames fanned by sensation-hungry journalists.

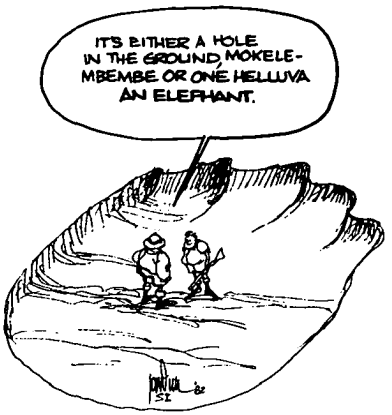
* * * * *

The scientific expedition headed up by Dr. Roy Mackal that went off to the Congo to pursue reports of dinosaurs still living in a remote jungle area has returned, alas, without any convincing evidence of what the natives call "Mokele-Mbembe." The twelve members of the expedition, including pygmy bearers, sometimes went slogging through mud up to their waists, and became covered with insect bites from head to foot.

Mackal, a University of Chicago biologist, told the press that, although

no member of the expedition actually saw Mokele-Mbembe, they did see footprints that he did not believe could have been caused by any known animal. However, Richard Greenwell of the University of Arizona, the expedition's anthropologist. and zoological collector, described them not as "footprints" but as "indentations," which in his opinion were too indistinct to rule out their being caused by an elephant.

Meanwhile, Herman Regusters, an engineer from South Pasadena, California, after unsuccessful efforts to join the Mackal party, formed an expedition of his own to Lake Telle. He reports that *his* group actually saw a dinosaurlike creature stick its head up three times out of the water and dive back down. But Greenwell doesn't accept Regusters's claim, principally because the deepest part of the lake is only nine feet. While the Mackal expedition was in the Congo, they learned that they had unfortunately misidentified the location of the Tebeke River, where the pygmies report the most



sightings of Mokele-Mbembe, and hence may have been barking up the wrong tree; Greenwell hints at another expedition. In any case, the expedition's soil samples, its collection of insects, snakes, bats, and monkeys, and its discovery of an archaeological site, made the trip successful from a scientific standpoint, even if Mokele-Mbembe continued to outwit its pursuers.

—*Robert Sheaffer*

Paraphysicists and deception

There is no indication that paraphysicists have the slightest desire to train themselves in the subtle arts of deception, or seek the aid of knowledgeable magicians in any significant way. As a result, they continue to act like excited little children every time a new psychic charlatan shows them a trick they can't explain.

—Martin Gardner, "Parapsychology and Quantum Mechanics," in *Science and the Paranormal*, George O. Abell and Barry Singer, eds., Scribner's, 1981.

Remote Viewing Revisited

Well-controlled experiments don't find the "RV effect," while poorly controlled ones do. These and numerous other problems lead to the conclusion that remote viewing is a cognitive illusion.

David F. Marks

The 1970s saw the emergence of an astonishing psychic phenomenon that the principal investigators called "remote viewing." This refers to an alleged ability to perceive information from remote sources not available to any known sense. Actually this product of the paranormal is not a new one—it is really good old faithful ESP in a new package and with a different brandname. The main promoters of remote viewing are two physicists at SRI International (formerly the Stanford Research Institute), Russell Targ and Harold Puthoff. The remote-viewing effect could apparently be obtained by anybody and it required no special training or unique abilities. The results were allegedly reliable and repeatable. In fact, remote viewing was every parapsychologist's dream come true.

This is how one observes remote viewing. Someone (let's call him the traveler, T) gets into a car and drives to a place some distance away (e.g., a park, a church, a city hall, a railway station, or a golf course). Someone else (let's call him P) waits with the subject (S) with a tape recorder and a drawing pad. At a prearranged time, when T will have arrived at the target location, S tries to describe the location using his or her imagination. Experimenter P may assist S by asking various questions to clarify the description. S may also draw a picture of the target location. T allegedly acts as a kind of "psychic beacon" beaming back information from the target using some unknown sensory modality. After a prearranged interval (say 15 or 30 minutes) of remote viewing by S, T returns and escorts S to the target site to provide feedback on how well S has done. The experiment

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is repeated a number of times (e.g., 9).

Generally speaking, S, T, and P are all highly delighted with the results and all kinds of matching elements are found between S's descriptions and aspects of the target sites. I can personally vouch for the reliability of the remote-viewing effect when evaluated by simple inspection of the target site immediately after S has produced his response. I have found the remote-viewing experience compelling and direct and, on occasion, eerie. However it is obviously important to validate the effect on a more objective level. To this end, S's taped descriptions are transcribed and, together with any drawings that may have been produced, the transcripts are given to a judge who tries to match them against the series of targets. The judge normally visits all the sites and ranks the transcripts at each site in the order of their degree of matching.

Targ and Puthoff (1974, 1976, 1977) have reported extremely successful results from this judging process, and so a compelling subjective phenomenon has apparently been quantified numerically in the form of probability values. The implications of remote viewing (if real) for science are enormous. Textbooks of psychology, physiology, anatomy, and physics would all need to be completely rewritten, as our knowledge of the bases of perception and psychophysics built up over the ages would have to undergo major revision. Before this revolution in science begins, however, it would seem prudent to examine the phenomenon of remote viewing more closely to determine whether there is any possibility of artifacts, flaws in the methodology, experimenter bias, or some other parsimonious explanation.

With this in mind I began an investigation in 1975, in association with Richard Kammann, the results of which were published in our co-authored book *The Psychology of the Psychic*. Like many other investigators we were unable to replicate the effect claimed by Puthoff and Targ, as none of our judges could match transcripts accurately. Following visits to SRI and multiple discussions with Targ, Puthoff, and their key judge, Arthur Hastings, it became clear that the SRI research program, promoted as well-controlled science, was actually a massive litany of fallacies and flaws. To avoid duplication, this article will take up the story more or less where our book left it two years ago. This review relates to the two series of experiments with Price and Hammid, and various attempted replications by other investigators.

Data Suppression

Science separates itself form pseudoscience along a number of dimensions. One of these dimensions is accessibility of the data. Scientific data are consensually validated by open inspection of the recorded observations or through replication of the relevant phenomena. Following publication of major observations it is an accepted practice in science for researchers to

allow colleagues who are doing serious research in the same field to have access to their original data. When researchers consistently refuse to allow colleagues such access, something important is being signaled. Of course data may get lost or destroyed or be difficult or costly to retrieve in the form required. Or they may be classified information or have commercial value that a scientist may wish to exploit prior to their general release. However, when none of these considerations is applicable, a refusal to supply a copy of a set of data leads to the unpleasant inference that something is wrong, that the data do not support what is claimed for them, that the data are an embarrassment following an extravagant claim that cannot be substantiated.

Over the past few years I have made frequent requests to Puthoff and Targ for copies of their remote viewing (RV) transcripts obtained in their experiments reported in *Nature* (1974), *Proceedings of the Institute of Electrical and Electronic Engineers* (1976), and *Mind-Reach* (1977). Targ and Puthoff have consistently refused to supply this important information, as they have to all others I know who made this request. The only concession has been to supply a single transcript from the Price series (Experiment 7), which is published in *Mind-Reach*. Normally no explanation or reply follows such requests. However in May 1979, while preparing the manuscript of *Psychology of the Psychic* I did receive the following explanation from Puthoff:

> With regard to your request, at the present time we are subjecting the transcripts to a number of blind judging procedures in order to assess which of several approaches constitutes the best way to handle free-response text in remote viewing studies. As a result we are not ready to release the materials, since premature disclosure would prevent further blind analysis work. I am sure you can appreciate the fact that after spending years building up a data base, we don't want to do anything that would jeopardize our options with regard to blind analysis of this data pool.
>
> By the way, this work is proceeding quite well. We have several research analysts breaking transcripts down into concepts which are then individually rated against concepts generated for the targets. . . .
>
> *When our blind analysis work has been completed, you may of course have access to the raw data.* I do not think it will be in 1979, however, as our analysis contracts extend beyond that; thus, I doubt it will be in time for your book." [Italics mine.]

Three years later I'm still waiting! Surely "several research analysts" are not still "breaking transcripts down into concepts." *I would like to publicly ask Targ and Puthoff to release all of their remote-viewing transcripts in their original unedited form.* This would enable members of the more skeptical scientific community to evaluate the data for themselves.

Targ and Puthoff supplied John Wilhelm with a single transcript (Price series, Experiment 4, Redwood City Marina), reproduced in Wil-

helm's book *The Search for Superman* (pp. 213-18). Although this was purported to be a complete transcript, it actually had two pages missing. A copy of the whole transcript given to me by Arthur Hastings (the SRI judge) contained nine pages. Pages 6 and 7 are missing from Wilhelm's version, and one can only speculate on the implications of the fact that one of Targ's most blatant cues ("Nothing like having 3 successes behind you") was on the missing page 7. Who could avoid the conclusion after reading this cue that this experiment was number 4 in the series?

The refusal to release the data-base for their controversial claims is a disturbing feature of Puthoff and Targ's remote-viewing project. The task of presenting an accurate and fair evaluation of the research is made none the easier as a consequence.

Fortunately, the main judge in the SRI research team, Arthur Hastings, was more helpful. Hastings loaned me a complete set of nine transcripts from the Price series and showed me six of the nine Hammid transcripts. However, Hastings said he felt "paranoid" about releasing this information and soon requested their return as they had become "confidential." How much longer these data will remain confidential is anybody's guess. It may be forever.

Sensory Cues

Most of the information in the RV transcripts is descriptive material, honest attempts by the subjects to describe the remote locations. Examination of the RV transcripts supplied by Hastings, however, revealed an enormous array of extraneous information and cues. Basically these are bits and pieces of information about the experiments that enable the judge to place the transcripts in their correct sequence: dates, times, references to previously visited targets, statements made by the experimenter monitoring the subject (including leading questions), and other information of this type. For both the Price and Hammid series there are enough cues to place the transcripts in almost perfect sequence. I could hardly believe my eyes when I saw that four of the six Hammid transcripts actually carried dates. Hastings told me one transcript even carried the name of the associated target, although he returned this one to SRI!

Being able to place a series of transcripts in correct sequence is useful only if the target sequence is also available to the judge. I was dumbfounded to discover that Puthoff and Targ gave Hastings a target list for the Price series in correct sequence. Hastings gave me a copy of the actual list he received from SRI. However, on 10 July 1978, Puthoff wrote me a letter in which he stated: "With regard to your assumption that the judges knew the order of target visitation, that is incorrect. Neither the single judge (Hastings) nor the panel of five judges were given the order; they were kept blind." This information was quite false. Two years later, Puthoff retracted it: "You are correct that, in the case of the Price series,

the published list of the targets was in the order of target usage, and that, furthermore, when the target list was provided to the judge, it was inadvertently (through an error) given in the same order" (letter from Puthoff, August 25, 1980).

Using only the extraneous information in each transcript, together with the same list of target names provided to Hastings, independent judges in New Zealand (about 12,000 km from the target sites) were able to blind-match the transcripts against the Price targets with exactly the same degree of accuracy as that achieved by Hastings. I have called this phenomenon "remote judging" (see Marks and Kammann 1980, pp. 29-31, for more details).

The same excellent results from remote judging have been obtained from the Hammid series. The Hammid series is a little more complex than the Price series but basically the remote-judging results are as good as those obtained by the original SRI judge, as shown in Table 1.

It is important to remember that the three remote judges had access only to the cues provided in the Hammid transcript together with the target lists and the map provided to the SRI judge. No site visits were possible and none of the descriptive material from the SRI transcripts was available.

To illustrate how the remote judging process works, a total of 24 cues found in six Hammid transcripts, together with the conclusions that can be drawn from them, are shown in Table 2.

In the light of Table 2 the following claim (Tart, Puthoff, and Targ 1980) is very difficult to believe: "In the extensive replication studies, which

TABLE 1

Distribution of Rankings Assigned to Transcripts Associated with Each Target Location in the Hammid Series

Target	Remote Judges			SRI
	A	B	C	Judge
Methodist Church	2	2	1	1
Merry-go-round	2	2	1	1
Parking Garage	1	1	1	2
Railroad Trestle Bridge	2	1	2	2
Pumpkin Patch	2	1	2	1
Pedestrian Overpass	1	1	1	2
Total sum of ranks	10	8	8	9
p value	.005	.001	.001	.002

TABLE 2

Sensory Cues Available in the Transcripts for the Hammid Series and Conclusions That Can Be Drawn from Them about the Experimental Sequence

Experiment Number	Transcript[a] Number	Cue No.	Text Phrases Relevant as Cues	Conclusions
1 (Courtyard)	Not available[b]		(Almost certainly mentions date and/or time. Other cues?)	(First target?)
2 (Auditorium)	Not available[b]		(Date and/or time? Other cues?)	(Second target?)
3 (Merry-go-round)	3	(1)	The time is 11:07	Possibly first of the day.
		(2)	This is the 18th of September	Target early in series.
		(3)	Russell Targ has gone to the remote site	Target possibly outside SRI.
		(4)	"My mind is unfortunately clouded by his parting words when he kissed me goodbye and said 'I'll see you in church.' I wish he hadn't said that."	Maybe the church target or a target to be visited near the church (merry-go-round?).
		(5)	"All the various subliminal images I discounted yesterday."	Not first target of series.
		(6)	"I keep seeing him riding his motorcycle maybe because I know that he went there with a motorcycle."	Probably a target outside SRI (confirms cue 3).
		(7)	"His body isn't moving like it was yesterday."	Not first target of series (confirms cue 5).
4 (Church)	8	(8)	September 18th . . . Russell Targ at the location.	Target close to target associated with transcript 3?
		(9)	"Is this going to be one of those where you tell me or not?" (Hammid's question.)	Not first target of series.
		(10)	"We haven't decided yet. We haven't given you any feedback today?" (Puthoff's answer and question.)	Second target of the day.
		(11)	"No we only did one." (Hammid's reply.)	Target must follow target associated with transcript 3.
5 (Pumpkin patch)	6[c]	(12)	October 11	Target must follow targets associated with transcripts 3 and 8.
		(13)	a.m.	Target precedes target associated with transcript 9.
		(14)	Reference to Hal (Puthoff) as visitor to target? (Other cues?)	
6 (Railroad bridge)	9	(15)	"Our second experiment of the morning."	Target immediately follows target associated with transcript 6.
		(16)	"It is a quarter to twelve."	
		(17)	"Hal has gone to a second target."	
7 (Overpass)	2	(18)	October 11, 1974.	Target immediately follows targets associated with transcripts 6 and 9, but must have at least 2 afterwards—*must be the 7th target.*
		(19)	"Hal has gone to the first of three remote sites that he will visit in this experiment."	
		(20)	"At 3:45 Hal will be at his first target selected outside SRI."	
		(21)	"—I have something in mind too—maybe its because I noticed it on the way back when I looked at the pumpkin field."	Target must be after the pumpkin patch. (Confirms cues 18-20.)
8 (Parking garage)	7	(22)	"Hella has made a drawing of Hal's first location."	Target immediately follows target associated with transcript 2.
		(23)	Four-fifteen to four-thirty.	
		(24)	"Is it a new thing—or from the previous site?"	
9 (Bicycle shed)	Not available[b]		(Almost certainly mentions date and/or time. Other cues?)	Transcript must be the last of the series (see cues 19, 20, 22-24.)

Notes: a. Transcript identifiers were randomly assigned.

b. Dr. Hastings would not consent to the release of these transcripts.

c. This transcript was available for a cursory inspection only and therefore may have contained other cues unlisted in this table.

also yielded significant results, the Marks-Kammann criticisms do not apply in principle. Target lists and transcripts were separately randomized, and transcripts were carefully checked before judging to ensure absence of any phrasing for which even a weak post-hoc potential-cue argument could be made." How careful was a checking process that missed 24 cues in six transcripts?

The claim that the Hammid target list given to Hastings was randomized is also highly moot. It actually depends on which list one is talking about, because, although the SRI researchers appear unwilling to admit this, no less than *three* listings of targets in the Hammid series were given to the judge. *"I received three target lists"* (letter from Hastings, May 26, 1977). One of these lists was randomized; this is the one cited by Puthoff as *the* (implying *only*) target list given to the judge. The other two lists provided by SRI (described in detail by Hastings in his letter to me of May 26, 1977) were not random. One of the two nonrandom listings was "a series of pages, each with the name of a target and location. The order of those pages, as I now have them, is this: courtyard, auditorium, Methodist church, playground, overpass, parking garage, railroad bridge, pumpkin patch, bicycle shed" (Hastings's letter, May 26, 1977). The correlation of this target listing and the order of target usage is 0.833 ($p<.01$). This listing therefore would have provided an artifactual basis for correct matchings (see Marks 1981a).

The second nonrandom listing of Hammid targets given to the judge by SRI was a map of the Menlo Park/Palo Alto area indicating the location of the nine target sites. There is a strong correlation between map codes and pairs of targets visited outside SRI by Targ or Puthoff on different days and half-days over the series of experiments.

Here is a verbatim description from a remote judge of how he matched the six Hammid transcripts against the targets:

> The six transcripts fall into three pairs. The first pair are 8 and 3 [see Table 2]. Russell Targ is the traveller to both, on the same day, September 18. The give-away phrase in 3 is "I'll see you in church." Therefore I would say 3 is the Church and 8 is the Merry-go-round. The remaining two pairs are 6 with 9 and 2 with 7. Puthoff is the traveller. 6 and 9 were apparently visited one immediately after the other, on the morning of October 11. Looking at the map 6 and 9 either go with the Pumpkin Patch and the Railroad Bridge, which are very close together, or the Overpass and the Parking Garage, which are further apart. However this pair (6 and 9) must go with the Pumpkin Patch/Railroad Bridge combination because of the cue phrase in transcript 7 which mentions the "pumpkin field" as having already been visited. Therefore 6 and 9 go with the Pumpkin Patch/Railroad Bridge, and 2 and 7 go with the Overpass and the Garage. I am unsure of the order but I'll stick to the following: (6) Pumpkin Patch, (9) Railroad Bridge, (2) Overpass, (7) Parking Garage."

This remote judge (Judge B in Table 1) gained a sum of ranks of 8 giving a p

value of 0.001. This result achieved at a distance of 12,000 km and with no ESP ability—just plain logical thinking—exceeded that obtained by Hastings. Clearly the 24 cues available in the transcripts, the presence of which Targ, Puthoff, and Tart have denied, are sufficient to account for the entire RV effect claimed for the Hammid series.

This review of the SRI RV research unfortunately remains incomplete owing to the unavailability of the majority of raw data. Real difficulties can occur in evaluating research on the basis of incomplete information, and misinterpretations and errors may occur. An example of this can be found in *The Psychology of the Psychic* in relation to a mistaken inference concerning the Hammid experiments, which were hypothesized to have been selected from a longer series. Although training experiments were run prior to the published experiments, the allegation of data selection has been retracted (see Marks 1981b). However, I remain undecided about whether all of the drawings produced by Hammid were submitted for judging, since drawings were missing from some of the transcripts given to me by the SRI judge. But this is a minor point in the light of the remote-judging controls. The SRI experiments with subjects Price and Hammid can be seen as providing no evidence whatsoever for remote viewing. Contrary to Targ and Puthoff's claims, the quality of the subjects' descriptions is extremely poor and, without the cues, cannot be matched against the targets.

Replication Attempts

Attempts to replicate RV fall into two distinct groups. First there is a group of carefully controlled studies that avoided the flaws present in the Puthoff-Targ experiments and that found no evidence of RV (Allen et al. 1976; Rauscher et al. 1976; Karnes et al. 1979; Karnes and Susman 1979, 1980; Marks and Kammann 1980). When all normal or artifactual methods for matching the subjects' descriptions against target sites are eliminated, it is apparent that the RV effect completely diappears.

A second group of experimenters claim to have confirmed RV. However, all of these experiments are flawed in a variety of ways, some of them reminiscent of the original SRI research and some new. The size of the RV effect claimed in these studies actually correlates quite well with the magnitude of the flaws present.

Hastings and Hurt (1976) reported a single RV experiment that used as a target "a circular play area, filled with sand, and containing a log structure with chains hanging from it and a slide on one side. ... There were swings next to the play area, and a jungle gym alongside the sidewalk." A group of 36 subjects attempted to describe this target, which was selected from a set of six possible targets all within a 10-minute drive of the "laboratory." The names of the targets were written on cards, which were sealed inside envelopes. One envelope was randomly selected by

throwing a die, and David Hurt and a companion then departed with the sealed envelope. The envelope was opened in Hurt's car and the two travelers proceeded to the target site and spent 10 minutes there. Apparently the other five envelopes containing the names of the nontargets were left behind with the other experimenter (Arthur Hastings) and the group of subjects. It is a matter of some speculation what Hastings could possibly have discerned from the five envelopes left in his control. However, before the remote team returned, no less than 20 of the 36 participants had selected the correct target site from the six available alternatives named and described by Hastings. This procedure seems incredibly lax and poorly controlled and the potential for cueing and bias in the subjects' descriptions is enormous. To quote the experimenters' report: "We think that the effectiveness of the experiment was partly due to the way we conducted it." Hastings and Hurt recommend that in future research "the target pool should be made up by someone not at the experiment." Who would not agree?

Whitson, Bogart, Palmer, and Tart (1976) reported a study not dissimilar from the Hastings-Hurt experiment just described. A target was randomly selected from a set of ten sites. A group of 27 art students attempted to draw this site while it was viewed by one of the experimenters. A judge was asked to match a first and second choice from a set of 10 slides of the sites projected simultaneously on a screen. A bias was observed in the judge's rankings favoring the target site, a bike tunnel. A second experiment was run using another target and another art class of 14. The results of the second experiment displayed a drawing bias but, on this occasion, favoring a different site, which was not the target. This study illustrates an important control for RV research—it is essential to control for biases operating in the subject's responses. The next study provides a potent illustration of what can happen when response biases are ignored.

Vallee, Hastings, and Askevold (1976) reported a series of 33 experiments using a computer-conferencing network with 12 participants in various locations across North America. At prearranged times the participants used portable computer-terminals located in their homes or offices to type descriptions of a mineral sample, selected from an available set of 10. Eight out of 33 descriptions were correctly matched by a panel of five judges, compared with a chance expectation of 3.3.

The implications of this result (if true) for human perception are absolutely mind-boggling. Apparently human beings can discriminate a baseball-sized object at a distance of 2,500 km, differentiate the object from nine others in an arbitrary and unknown set, through an incalculably huge number of physical barriers, and without interference from a huge landmass of rock and minerals in the North American continent. Before rewriting our textbooks on perception it would pay to examine the details of this experiment a little more closely. In fact we discover that although 12 participants took part in the experiment, results for only 6 subjects are

included in the experimental report. Of the 8 matching descriptions, 3 were obtained for target D (opals) and 2 for target F (common salt crystals). The results become a little less surprising when one notes the strong bias in the subjects' descriptions (as rated by the judges) toward opals and salt, regardless of which mineral was the target. The results for these two minerals when they were targets gave 5 correct matches in 10 attempts (50 percent). However, for the remaining 23 experiments, when samples D or F were *not* the target, 8 (or 35 percent) of the descriptions also matched D or F more closely than any other. When the results are considered in terms of rankings instead of the peculiar rating system used by the Vallee team, we find that the average ranking of the subjects' descriptions against samples D or F when D and F were targets was 2.80, and when they were not targets, 2.96, a minuscule difference. The results for the other 8 targets gave 3 correct descriptions in 23 attempts, almost exactly what we would expect purely by chance. Therefore, having corrected for response bias, these results lack any real substance in their support of RV.

Dunne and Bisaha (1979) conducted precognitive RV experiments in which the subjects allegedly described the targets *before* they were selected. In the first series of seven experiments subjects worked in pairs but independently. Dunne and Bisaha reported results for seven subjects (S4–S10). For some unexplained reason the results for the three other subjects (S1–S3) are missing from the experimental report. Also ten targets were selected, but the results for only seven are presented. The protocol differed from the SRI research in that photographs of the targets were used by the judges rather than on-site visits. Apparently several photographs were taken at each target site and a selection must have then been made, as only one photograph of each site was given to the judges. Since the photographs used in judging must have been selected *after* the subject had described the target, unless the selection was conducted by an independent third party, the experiment is worthless. The selection could obviously have been biased by the experimenter's knowledge of the subject's descriptions.

Dunne and Bisaha randomly divided the total of 14 transcripts into two sets, Group A and Group B. Two judges were asked to blind rank Group A, two judges to blind rank Group B, and two judges matched Group A transcripts against Group B transcripts. John Bisaha sent me the transcripts for this experiment and they do contain a few cues, although not on the same scale as the SRI transcripts I have seen. For example, transcript A-5 is labeled "Barb 5/9," the subject's name and the date. Transcript B-2 is labeled "Steve B 5/9,". Needless to say these two transcripts were correctly matched. Evidence of post-hoc data-selection is also present in one of the transcripts. The experimenters claim to have attached any drawings the subject made to the transcripts for judging. Yet Transcript A-2 from subject 5 for the Madonna del Strada target included no drawings even though the transcript states in two different places that a

drawing was done. Why was this drawing excluded? Was it a poor match?

An RV report claiming confirmation of the SRI research was published by Schlitz and Gruber (1980). A target in Rome was visited on each of 10 days at a prearranged time, while a second experimenter in Detroit sketched and wrote down her impressions as she concentrated on the distant target. Transcripts, translated into Italian, together with associated drawings, were given to five judges who visited the 10 sites and then ranked and rated the 10 transcripts against each target. The resulting scores were highly significant for four out of the five judges.

Schlitz and Gruber state that no cues were found in the transcripts so that no editing was necessary. However the drawings were attached to the transcripts following their translation into Italian. Were the drawings and transcripts dated or coded? If not, how could they have been put together correctly following the translation? What sort of coding was used? How were the targets listed when given to the judges? Were they in the correct order or in random order? Unfortunately we are not told this vital information and are left with an uncomfortable feeling of déjà vu.

Conclusion

Well-controlled experiments never find the RV effect, while poorly controlled experiments nearly always do. Data suppression, flawed methodology, and lack of replication lead to the conclusion that remote viewing is a cognitive illusion, an artifact of human error and wishful thinking.

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On the "Correlation" Between Radio Disturbances and Planetary Positions

Comparisons of planetary position with even totally arbitrary sets of data, such as satellite decay dates, show unavoidable central peaks when J.H. Nelson's methods are used. The reason why is easy to show.

Jean Meeus

In his famous paper in *RCA Review* (1951), J. H. Nelson claimed to have found a relation between, on the one hand, the conditions of radio communication across the North Atlantic, and, on the other, certain geometrical aspects of the six inner planets (Mercury, Venus, Earth, Mars, Jupiter, Saturn).

According to the quality of the radio signal, a given day was called "normal" or "disturbed." The astronomical data were the differences of heliocentric longitude between the six inner planets taken in pairs. Whenever any one of these differences was 0, 90, 180 or 270 degrees, a "configuration" was said to occur. Thus, for example, a day when Mars was 90 degrees in advance of Mercury in its orbit, or when Jupiter and the Earth were in line with the sun, was called a configuration day.

Nelson compared these two sets of data for the years 1942, 1944, 1947, 1948, and 1949. Calling a configuration day zero, each day on which reception was rated as "disturbed" must occur either at zero or 1, 2, 3, . . ., days before or after zero. In this way a frequency distribution was built up by plotting the number of disturbed days against days before or after a configuration day. For an example of such a graph, see the SKEPTICAL INQUIRER, Fall 1981, page 34. Such a peaked curve seems impressive indeed. Was it a definite proof that days of disturbed radio propagation tend to coincide with configuration days?

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As has already been pointed out by Hunter (1952), however, the "correlation" found by using Nelson's procedure is illusory. If you have a set *S1* of events *happening quite randomly in time* (for instance, planetary configurations) and another set *S2* of events (for instance, radio disturbance days), and if you then associate each day of the second set with the nearest day of the first, you will obtain a distribution curve with a central peak just as impressive as Nelson's curves, *whether or not the events are causally connected.* Your technique of counting any date of set *S2* from the nearest date of set *S1* is not justified; it automatically results in a peaked curve!

More recently, Ianna and Margolin (1981) obtained a similar peaked curve by using *simulated* data. Their randomicity tests show without any doubt that the apparent relation between radio disturbances and planetary positions is an *artifact* of Nelson's counting method.

We have imagined yet another association of sets of data in order to show that we once more obtain the unavoidable peak when Nelson's procedure is used.

Set *S1:* the alignments of a planet (Mercury to Pluto) with the sun as seen from Earth, that is, each date when one of the planets is in conjunction with or in opposition to the sun as seen from Earth.

Set *S2:* the dates of decay of the Russian Cosmos satellites. At the end of 1981, a total of 1330 Cosmos satellites had been launched by the USSR, of which 846 had returned to Earth—268 by natural decay and 578 were recovered.

Each of these Cosmos decays was then associated, as in Nelson's method, with the nearest date of the first set. In this manner, each Cosmos decay gave a value for *Dt,* the time difference in days. All the dates were based on the Universal Time, or Greenwich Civil Time. A sample of the list thus obtained is shown in Table 1.

The reader can construct the complete list himself, if he wishes to check our results. The dates of the Cosmos decays can be found, for instance, in NASA's *Satellite Situation Reports,* while several astronomical almanacs will provide the dates of the conjunctions with and oppositions to the sun.

The extreme values of *Dt* were -34 and +33 days, respectively, and the distribution of the *Dt* values is given in Table 2. Taking the *Dt* values by groups of three, the graph shown in Figure 1 is obtained. We obtain a peaked curve, yet we can hardly believe that the Soviets looked to the planetary positions before recovering their satellites!

But what is the actual reason that the obtained distributions show a central peak? Neither Hunter nor Ianna and Margolin make this clear, yet the explanation is not difficult to find.

Suppose that during a given month there are only two configuration days, for example, days 12 and 15. Then the *Dt* values for days 6 to 22 are those given in Figure 2. In this case, we see that *two* days have *Dt* = 0, *two*

TABLE 1

Sample of the List

Cosmos No.	Date of Decay	Planetary Configuration and Date		*Dt*
366	1970 Oct 13	UR *c*	1970 Oct 2	+11
368	1970 Oct 14	UR *c*	1970 Oct 2	+12
369	1971 Jan 22	ME *i*	1970 Dec 28	+25
370	1970 Oct 22	ME *s*	1970 Oct 27	-5
373	1980 Mar 8	ME *i*	1980 Mar 6	+2
376	1970 Nov 12	SA *o*	1970 Nov 11	+1
377	1970 Nov 23	NE *c*	1970 Nov 23	0
378	1972 Aug 17	ME *i*	1972 Aug 7	+10
380	1971 Jun 17	ME *s*	1971 Jun 21	-4
383	1970 Dec 16	ME *i*	1970 Dec 28	-12

UR = Uranus, ME = Mercury, etc.
c = conjunction with sun, *o* = opposition, *i* = inferior conjunction, *s* = superior conjunction

TABLE 2

Dt	Number of cases	*Dt*	Number of cases	*Dt*	Number of cases	*Dt*	Number of cases
-34	1	-17	7	0	42	+17	7
-33	0	-16	5	+1	47	+18	6
-32	1	-15	6	+2	36	+19	4
-31	0	-14	8	+3	32	+20	6
-30	0	-13	14	+4	37	+21	1
-29	0	-12	20	+5	26	+22	3
-28	0	-11	13	+6	24	+23	5
-27	0	-10	20	+7	20	+24	2
-26	1	-9	21	+8	28	+25	5
-25	2	-8	29	+9	18	+26	1
-24	0	-7	26	+10	21	+27	1
-23	3	-6	30	+11	21	+28	1
-22	2	-5	33	+12	16	+29	0
-21	3	-4	27	+13	14	+30	0
-20	5	-3	28	+14	10	+31	1
-19	6	-2	35	+15	12	+32	1
-18	4	-1	38	+16	9	+33	1

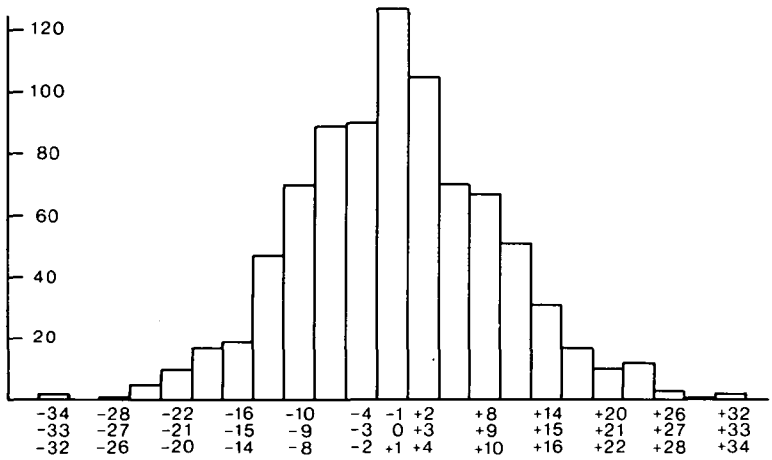

FIGURE 1. Histogram of the interval *Dt* between Cosmos satellite decay days and the nearest planet-sun alignment days.

days have $Dt = -1$, and *two* days have $Dt = +1$. But there is *only one* day for each of the values -2 to -6, and +2 to +6. Hence, any day taken at random has a higher probability to correspond to a small value of $|Dt|$ than to a larger one. The same holds for other distributions of the configuration days, resulting in more days near $Dt = 0$.

date	6	7	8	9	10	11	(12)	13	14	(15)	16	17	18	19	20	21	22
Dt	-6	-5	-4	-3	-2	-1	0	+1	-1	0	+1	+2	+3	+4	+5	+6	+7

FIGURE 2. The explanation of why there are more days with a small value of *Dt* than days with a large one.

The necessary condition for obtaining a peaked curve is that the events of set *S1* (to which those of set *S2* are compared) should not be equally spaced, and some fraction of them should be closer together than twice the interval of days counted around the configuration days. This is the case for planetary alignments, because there are several planets and each of them moves independently of the others. If, however, the dates of the lunar phases (new moon, etc.) are chosen for set *S1*, then the resulting curve will show no peak at all because the phases of the moon are distributed almost evenly in time, resulting in the same number of dates with $DT = -2$, for instance, as dates with $DT = 0$.

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Two Tests of Divining in Australia

Water at the divined depth and a diviner's own gold ingot both elude discovery

Dick Smith

I. Report on Water-Divining Test

After seeing a "teaser" on divining for the "Willesee Show," a popular television program in Sydney, Australia, Malcolm McDowell, a property owner in an outer suburb, decided to use the services of Vic Vaisey, one of the most experienced diviners in the area. (Vaisey was one of those who participated in the divining tests James Randi and I conducted in Sydney a week later for the Willesee program.)

Knowing my involvement with the Sydney tests, McDowell rang me up to ask if I would like to follow the proceedings. I enthusiastically agreed, since most of the diviners we had tested (including Vaisey) said they earned their living from divining and had never had a failure. We had heard a lot of anecdotal evidence, but this was an opportunity to follow such a claim from start to finish. The test was conducted in September 1980.

McDowell desperately needed additional water for his stock and for irrigating his five-acre parcel of land. He had previously written to the New South Wales Water Resources Commission, who then requested a map of his property. The Commission advised him in writing that water might be found at a minimum depth of 15 meters (50 feet), most probably at 60 meters (200 feet), and at a maximum depth of 100 meters (330 feet). No one from the Commission visited McDowell's property.

Vaisey went to McDowell's farm and spent about an hour applying

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Vic Vaisey performing at the original divining tests in Sydney.
Photo by James Randi, courtesy of Dick Smith.

his divining rod. He was not told about the Commission report. He divined two "definite" streams—one running east-west at a depth of 35 feet and another running north-south 5 feet deeper. (It should be noted that the dowser thus gave himself two chances of success in one spot.) He marked this location (within a few inches of where "the two streams" crossed).

Vaisey was quite definite that, since he had always had 100 percent accuracy, water would be found at this location and at the specified depth. He charged McDowell $100 for his service. (The Water Resources Commission had charged nothing.) He suggested that McDowell drill to 35 feet and, if the flow from that stream was not sufficient, that he press on the extra 5 feet to tap the next stream.

The drilling rig arrived on the site soon afterward. Because of a tree stump near the divined location, the rig was positioned to drill about six inches away from Vaisey's marker. I had suggested to McDowell that he make sure the hole was drilled exactly on the marked position; many diviners had claimed that their only failures were when the drill hole was not made in the exact spot recommended, causing the bore to miss the stream by a few inches! Therefore McDowell insisted that the rig be moved around the stump in order to drill the hole in the exact location. The drill operators were amused by this request.

The drill quickly went down to the 35-foot and 40-foot levels, and farther, only to find hard, dry shale to over 80 feet. Water was eventually struck at 210 feet at 130 gallons an hour. An increased flow of 250 gallons

an hour was struck at 220 feet. Further drilling to a depth of 253 feet produced no extra flow.

These results were entirely consistent with the Water Resources Commission predictions, even though they had not visited the property. McDowell then contacted Vaisey, who could offer no explanation for his failure. He said it was the first time he had ever failed. McDowell hinted that his $100 should be refunded, but Vaisey made no offer.

II. Report on Metal-Divining Tests

Following the dowsing tests that James Randi and I had made in Sydney, there had been an outcry from diviners in Perth. They claimed they could easily divine metal hidden in a box with 100 percent accuracy.

A well-known Perth radio station, 6WF, decided to do a series of tests of diviners and then send the most successful one to Sydney to claim the $40,000 I had offered for a successful demonstration.

On September 5, 1980, twenty-five of Western Australia's diviners performed in Perth in front of a large public audience and television cameras. All of them said they could easily perform under the stated conditions (cardboard boxes laid out on the ground, with one containing a piece of metal). All said they would complete an agreed number of tests.

The protocol could hardly be called good, nor could the test conditions be called scientific. However, the results were predictable. Even though a number of the diviners stopped short of the required number of tests when they found they were failing, the average score was 18 percent—the result from chance alone would have been 20 percent.

The diviners gave various reasons for failing, from an alleged huge aluminum deposit under the ground to the jewelry worn by onlookers.

The diviner with the highest success rate for the day, Cecil Holmes, of Gosnells, a Perth suburb, was flown to Sydney by the radio station for testing by me.

Holmes arrived at my home with his wife on the morning of Saturday, September 13. He immediately produced a small gold ingot, dropped it on the ground, and enthusiastically showed how he could divine it. When I pointed out that, since he knew where the gold was, it was not a good demonstration of divining, he laughed and said that it didn't make any difference because it was the gold pulling the rods, not his knowledge of the gold's location. Like most believers in divining, Holmes and his wife were obviously sincere.

After I explained the test conditions (a row of ten boxes, with the gold ingot hidden under one of them), Holmes said he would easily be able to obtain at least 80 percent accuracy. I asked Mrs. Holmes to be the chief judge. The other two judges were Trevor Munnery, a reporter from the newspaper *Truth,* and Garry Crapp, a business associate of mine.

Before the test started, an unusual situation occurred. As is traditional in such tests, in full view of Holmes I placed the gold ingot in a box and

asked him to see if his powers were working. Instead of going directly to the chosen box (as we expected him to), he walked up and down the row of boxes showing us that he received no reading from the empty boxes. He then mistook the correct box to be the one next to it and promptly divined the wrong box. His wife called out to him to "remember which box it was put in," but to no avail. After this bad start, we went ahead with eleven different tests. At the conclusion of the tests, he was asked how he thought he had fared. "At least 80 percent, probably higher," he said. His wife then announced the results—one hit in eleven tries—which were entirely consistent with chance.

In spite of his failure, Holmes then gave a demonstration of another of his powers—that of divining a two-dollar bill. He showed us that, if he held a one-dollar bill and approached a two-dollar bill lying on the ground, the divining rods would not move, but that they would if he held a two-dollar bill. He was amazed to find that this power would not work when we covered the two-dollar bill on the ground with a piece of paper. He had not bothered to try such a test before because he had not thought it necessary.

At the conclusion of the tests, Holmes said he thought they had been extremely fair and could not explain why his powers had not worked. Although he still had faith in his divining abilities, it was obvious that Mrs. Holmes had her doubts. •

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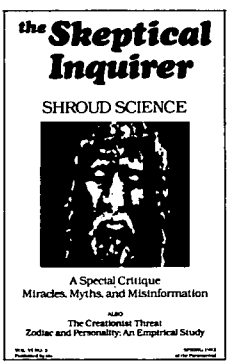

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The "Great Lakes Triangle" Pseudomystery

The lakes don't form a triangle, but do they pose a supernatural hazard?

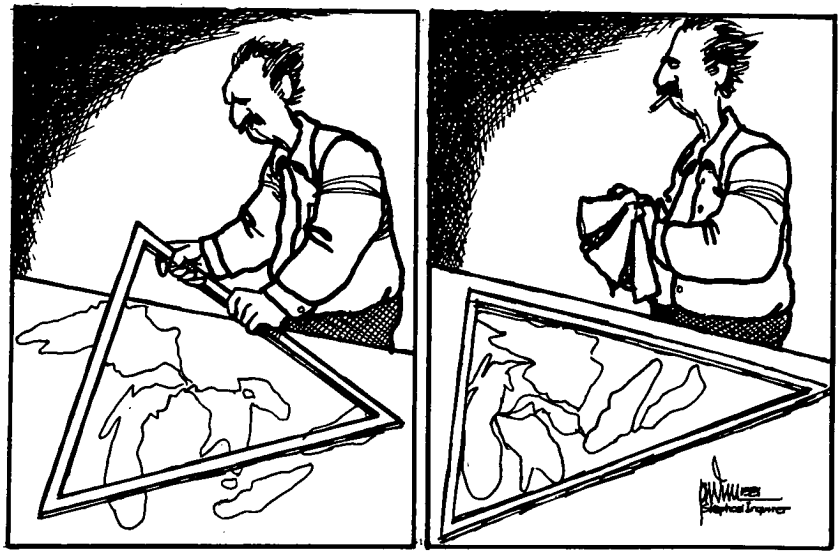
Paul Cena

There is a great interest these days in stories of the occult and mysterious phenomena such as ghosts, flying saucers, and strange sudden disappearances. In an attempt to cash in on this, writers keep churning out books on these subjects, depending upon which are in vogue at the time. There has been so much written about the "Bermuda Triangle"—an area in the South Atlantic where ships and planes, and their passengers, have supposedly vanished from the face of the earth—that such authors are apparently faced with a new problem: "What do you write about when you've run out of these disappearances?"

Well, the solution is simple. You just find another area with a large body of water and you invent another "triangle." Hence, *The Great Lakes Triangle,* by Jay Gourley. In discussing this book I shall explain some actual disappearances by using factual sources. I also shall show how this author uses certain methods and some creative writing to mislead his readers. I do not attempt to explain all the disappearances. They are too numerous, and in many cases too little information is given to research the event. As a pilot, I can give some insight into the causes of airplane crashes in this area and into phenomena that are well known to pilots.

First, let's take a look at the Great Lakes. According to the *World Book Encyclopedia* (1974), Lake Erie is 240 miles long and from 38 to 57 miles wide. It has an area of 9,910 square miles. Because it is shallow—only 210 feet at its deepest—"the lake is quickly stirred by storms and often has violent waves." Lake Huron is 206 miles long and 183 miles wide, with an

Paul Cena is a pilot who lives and flies in the vicinity of the Great Lakes. His home is in Lockport, New York.



area os 23,050 square miles. "Violent storms that frequently occur between December and May make Lake Huron dangerous for shipping during the winter." Lake Michigan is 307 miles long and 118 miles wide at its widest point, with an area of 22,300 square miles. Lake Ontario is 193 miles long, 53 miles wide, and has an area of 7,550 square miles. Lake Superior is 350 miles at its greatest length, 160 miles at its greatest width, and 1,333 feet deep at its deepest and covers 31,700 square miles. It is the largest body of fresh water in the world.

According to *Compton's Encyclopedia*, "The lakes ... breed sudden fogs and affect the behavior of passing storm centers in a way that is difficult to predict. Strong winds may suddenly whip up the shallow waters along coasts into choppy breakers ... In winter the storms that sweep the lakes match in destructiveness those of the Atlantic Coast."

So we are not dealing with small, calm lakes. These are large, sometimes violent bodies of water often marked by strong winds and hazardous flying and boating conditions.

Some events reported in *The Great Lakes Triangle* have a foundation in reality, but that reality is not explained to the reader. The author tells of some strange incidents but does not suggest that they could have logical explanations. The following legend, as written by Gourley, is not only exaggerated, it is an exercise in creative writing. He begins with the following:

> Ancient Chippewa superstition describes a giant sturgeon in Lake Superior. The huge legendary fish is capable of swallowing a whole ship. It can, with a flick of a fin, churn into boiling violence a small spot on the surface of the

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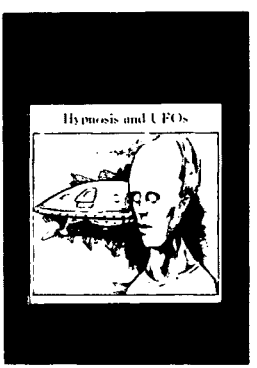

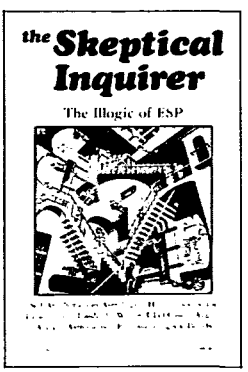

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lake ... The fish has not been seen by a white man who has lived to report it ...

The *Leafield* was a 4,453-ton freighter ... During the summer of 1913 ... without warning and with no indication of wind the water began to seethe and heave. Great seas from opposing directions poured over her from starboard and port ...

The *James E. Davidson*, a 6,000-ton grain-carrier was ... proceeding through a heavy snowstorm when [it] was confronted by a single, tremendous sea wave. It *thundered* aboard directly over the bow, *jarring* the ship to its very stern frame. In the firehold men were sent *tumbling*, dishes *cascaded* from the galley racks and the captain was *catapulted* from his bunk. The anchor windlass was torn from its bedplates and its controls *smashed*. Both anchors dropped from their pockets while tons of chain *writhed madly* about in the chain locker. [Italics added.]

In addition to using colorful language, Gourley writes as though he had been on board at the time.

Similar events occurred aboard the *Sachem* and the *Hibou*. After some quotes from Charles Berlitz's *Bermuda Triangle*, Gourley summarizes these events with another bit of sensational writing:

What force was behind the strange devastating wave? From where did it come? What power generated it? How could it be so accurately directed as to send a perfectly seaworthy ship crashing to the bottom? ...

Here he implies that somebody or something is purposely directing the waves at the ships. But, as I said, there is a logical explanation. According to the *World Book Encyclopedia:*

Seiche (pronounced saysh) is a long wave in the relatively shallow water of a lake, bay, or harbor. It may be anywhere from a few inches to several feet in height. High winds, a small earthquake, or atmospheric changes may cause a seiche. Minor seiches occur daily on Lake Geneva in Switzerland, and on Lake Superior. A recent seiche struck 25 miles off the Lake Michigan shoreline from Whiting, Ind., to Waukegan, Ill., on June 26, 1954. Eight persons were drowned by the wave, which was 10 feet high. High winds and a sudden rise in air pressure over the lake apparently formed the seiche.

As you can see, there is an explanation, but the author would have us believe otherwise.

One of the greatest blunders in Gourley's book is his account of the "mysterious" disappearance of the *Edmund Fitzgerald*. The legend, as told by the author, is as follows (compare the italicized phrases with the subsequent explanation):

On November 9, 1975, the *Edmund Fitzgerald* was loaded with 26,216 tons of iron ore ... When Captain McSorely guided the *Edmund Fitzgerald* out of Duluth-Superior Harbor, *seas were quiet*. Over the next 24 hours a

moderate storm developed ... The *Arthur M. Anderson* and the *Fitzgerald* were traveling in tandem ... The *Fitzgerald* was about 17 miles ahead ... Captain Cooper could not see the *Fitzgerald* because a *snow flurry* restricted his visibility. That was just *a few minutes.* The snow lifted. Captain Cooper could *see for 20 miles.* There was no *Fitzgerald* ... What incomprehensible force was strong enough to pull a 26,000-ton freighter instantly to her death ... ?

It sounds strange, doesn't it? And it's frightening if you believe that it actually happened that way. But the *Buffalo Evening News* of November 11, 1975, tells a different story.

No survivors were found in the 51-degree waters after the *Edmund Fitzgerald* abruptly dropped off radar screens around 7:30 P.M. Monday in 75 mph winds. Waves on the lake were reported as high as 25 feet. An oil slick, 2 lifeboats, a liferaft and other debris were found ... Bob Matthews, chief lockmaster, said his instruments indicated wind gusts of over 90 mph ... Local observers labeled the storm the worst in 35 years.

So there wasn't a "quiet" sea and a "moderate storm" that lasted "only a few minutes." It was a severe storm with gale winds and a ship loaded with over 26,000 tons of iron ore trying to survive on hostile seas.

This case is a prime example of the poor research, the omission of evidence, and the outright falsehoods found throughout the book. Gourley's sources often are sensational books—at one point he even quotes from the *National Enquirer.* This leads to a growth of the legend and further distortion of the truth.

The following report is an example of the lengths to which some unscrupulous authors will go to create an interest in their books. Here Gourley quotes from Donald Keyhoe's *Aliens from Space:*

Perhaps the strongest evidence that UFOs might be directly linked to the destruction of aircraft in this area is what UFO buffs call the Walesville, New York, case. Keyhoe describes the case as it began just southeast of Lake Ontario: "Just before noon on July 1, 1954, an unknown flying object was tracked down over New York State by Griffiss AFB radar. An F-94 Starfire jet was scrambled ... When a gleaming disk-shaped machine became visible he started to close in. Abruptly, a furnace-like heat filled both cockpits. Gasping for breath, the pilot jettisoned the canopy ... The F-94 ... smashed through a building ... Four died ... Five were injured."

Actually, the pilot and the radar operator had bailed out and landed safely. Gourley then quotes from J. Allen Hynek and Jacques Vallee (*The Edge of Reality*):

"It was a malfunction alright, but the malfunction was caused by a UFO ... They sent two jets up ... The two pilots said that they saw the thing and it was

coming close to them, and then they described something called a heat wave ..." Vallee later told me in a personal interview that the pilot was Lt. William E. Atkins, 24, and the radar pilot was Henry Coudon, 26.

Sounds strange, doesn't it? Being hit by a heat-wave while chasing a UFO. That's what the author would like us to believe, but that is not what really happened. It was not necessary to have a "personal interview" to learn the names of the pilots. They were included in a story in the *Buffalo Courier-Express* two days later. According to this story:

> ... On a routine training mission, they [the two planes] were ordered by radio to investigate a third, unidentified airplane. They checked and found the plane to be friendly and started to return to their base ... Fire broke out in the forward part of one of the [Starfires] and the pilot and his radar operator parachuted from their red-hot cockpit at 7,000 feet."

The two planes had not been scrambled to chase a UFO; they were already in the air on a training mission. They didn't see a UFO; they identified a friendly plane. And they weren't hit with a mysterious heat-wave; a fire broke out in the plane. This whole story seems so absurd that it is laughable. It was so poorly researched by Gourley that one should question the credibility of all of the reports in this book.

In reporting the following incident, Gourley attempts to link it with a force beyond our control:

> Shelly Frances O'Neil was sport-parachuting near Damascus, Ontario, on September 8, 1974, near the eastern shore of Lake Huron. She got a good opening shock and a good canopy at about 2,000 feet above the ground. Then she did something very odd. She released the canopy... The immediate cause of death was evident. No one has been able to explain why she released the good canopy and failed to deploy her reserve.

Maybe it was an accident. If a person drives his car into a bridge abutment, no one would be foolish enough to suggest that some mysterious force compelled the driver to destroy himself. Yet that is what Gourley is asking us to believe in this case. Parachuters, especially sport-parachuters, have a quick-release mechanism as part of their gear. All they have to do is pull a cord to release their canopy. Paratroopers were equipped with such devices during the war for use when coming down over water. Needless to say, they should be used with caution. I do not know what the explanation is in this case, but there *is* something missing—the Federal Aeronautics Administration's investigation report. After every incident of this kind, the FAA investigates the cause of the accident. Why was this report not included in Gourley's book? Perhaps it provides answers to some of the questions and would destroy the air of mystery the author has tried to create. Maybe the parachutist panicked; or maybe she didn't get a good opening and decided to cut away from the main chute but didn't have

enough altitude to successfully deploy her reserve.

Researching the events described in this book was no small chore. It was impossible in some cases because too little information was given. The author listed one event without giving the names of the pilots, the area where the plane disappeared, the date, or the registration numbers of the plane.

Even though it covers a period of well over 100 years, there are only about 112 cases mentioned (both ship and aircraft). Considering the vast area covered by the Great Lakes, I find this remarkable. I find it even more remarkable that many cases can be explained as easily as that of the *Edmund Fitzgerald*. Since time did not allow me to investigate every case, I must speculate on some that are not easily explained.

First, some facts about flying in this area. It is foolish for a pilot to fly over a body of water as large as any of the Great Lakes. Even though engine failure is rare, if it should occur the pilot would have to make a water landing. In a plane not equipped with floats, this is dangerous to say the least. During my flight instruction, I was told to fly *around* a lake, not over it. In many cases the author made much of the fact that no debris was found after an airplane disappeared. This is not unusual. According to my flight instructor, a small, light airplane would float for about 30 seconds. Also, the Great Lakes are not calm ponds. They are large bodies of water subject to severe storms and can swallow even ships, let alone a little debris.

Gourley also mentions some planes crashing within a few miles of their destination airport. For an explanation I turned to *Plane and Pilot*'s "Safety Corner":

> An analysis of recent aircraft accident statistics indicates that the stall/spin accident continues to take its toll among General Aviation pilots in virtually every kind of aircraft. Furthermore, it is a recognized fact that most of these fatal stall/spins occur in the traffic pattern where chances of recovery are almost nil.

A stall occurs when air-speed is not high enough for the airplane to maintain its altitude. A spin occurs when recovery from a stall is not soon enough. A pilot distracted by adverse weather can inadvertently let his air-speed drop. Because altitude is lowest when approaching a landing, a pilot may not be high enough to successfully recover from a stall.

Another cause of accidents is simple pilot error. After a pilot has been flying for a while, he or she may become overconfident. Many accidents occur when a pilot doesn't bother to get a weather briefing, or when a non-instrument-rated pilot is caught in instrument conditions. Pilot error leaves no evidence, and it may often look as though a good airplane and a qualified pilot crashed for no reason.

Carburetor icing also takes its toll among pilots. The temperature in the carburetor of an airplane can be as much as 70 degrees colder than the outside air temperature. So, even on a warm day, ice can form in an engine

if a pilot does not apply carburetor heat in time, and it will appear that a perfectly good engine quit for no reason.

Gourley thinks there is something strange about an airplane crashing without sending out a distress call. There isn't. During flight instruction, I was told that when confronted with an emergency situation the first thing to remember is to "fly the airplane." In most cases a pilot is too busy keeping his airplane at a safe altitude to send a distress call. Such a call would not make his job any easier; it would only make his position known so that rescue craft could find him. Most planes have special radios that send out a signal upon impact. This device is called an emergency locator transmitter, or ELT. So even if the pilot doesn't call for help, he can be sure that he will be located. But this radio doesn't work when the plane comes down on water and sinks. Therefore, most planes that go down over land are found, but those that go down in the Great Lakes are not.

FAA investigators do not speculate. If they are not certain what caused a crash, they use the words "undetermined reason." Gourley takes this to mean a mysterious force, but it simply means there is not enough evidence to determine the cause of the accident.

Along with the "mysterious disappearances," Gourley has included some reports of UFO sightings around the Great Lakes. It seems he began writing the book with the preconceived idea that UFOs are "real" and was blind to all the evidence about what really happened. He is so intent on having us believe his UFO theory that he withholds evidence and includes cases that didn't even occur over the Great Lakes.

Following is the entire Epilogue to *The Great Lakes Triangle:*

> In the course of researching and writing this book many have asked what really caused the events herein described. I do not know.

If he had done a better job of researching he might know what really happened. He could have checked with area newspapers and eyewitnesses, or at least looked at an encyclopedia, instead of relying on sensationalist literature. He gratefully acknowledged permission to reprint from the books of Donald Keyhoe, Charles Berlitz, and J. Allen Hynek and Jacques Vallee, and from *Ghost Ships of the Great Lakes* by Dwight Boyer, but these are not reliable sources when looking for facts.

Unfortunately, we will continue to be plagued by books of this sort and authors will continue to produce them as long as the public continues to buy them. ●

Skepticism, Closed-Mindedness and Science Fiction

How and why paranormal believers use threadbare rhetorical strategies to try to deflect responsible critical analysis.

Dale Beyerstein

In order to cover up the lack of evidence for the existence of paranormal phenomena, the proponents of belief in such things like to change the subject. Their strategy is: "The best defense is an offense." Instead of defending their claims against the charges made by skeptics that there is no evidence for claims of the paranormal, defenders of these beliefs often simply attack the skeptics by calling them closed-minded. Usually when believers hurl this term of abuse at nonbelievers they are just poisoning the well and have no rational argument to back up their assertion. Their assertion could be replaced by a phrase such as "Your mother wears army boots!" without changing the logical structure of their argument. The main purpose of this charge of closed-mindedness is to intimidate, to get skeptics to back off from their criticism. It has the further use of leaving the impression in the minds of those who are unfamiliar with the debates about the paranormal that only those people who refuse to look at the evidence that is manifestly there could fail to be convinced about the existence of paranormal phenomena. Thus the impression is left among those who are unfamiliar with the alleged evidence that *if* they looked at it they would be convinced. This strategy is in fact the best one open to proponents of the paranormal; they get converts to the belief that there must be something in all this talk about the paranormal without exposing them to the weakest link in the arguments for its existence: the evidence.

In section I of this paper I shall show how charges of closed-mindedness are irrelevant to the disputes about the paranormal, in that if they apply at all to any parties in the dispute they would apply equally well to those on *both* sides of the dispute. In section II, I shall expose a typical

Dale Beyerstein is an instructor of philosophy at Malaspina College, Nanaimo, British Columbia.

method used by defenders of the paranormal to beg the question about the existence of such phenomena. Given the absolute uselessness of charges of closed-mindedness in these debates, we need an explanation of the fact that they are used with such regularity. Thus in section III, I shall suggest one reason that so many believers in the paranormal think they are getting somewhere by charging skeptics with closed-mindedness.

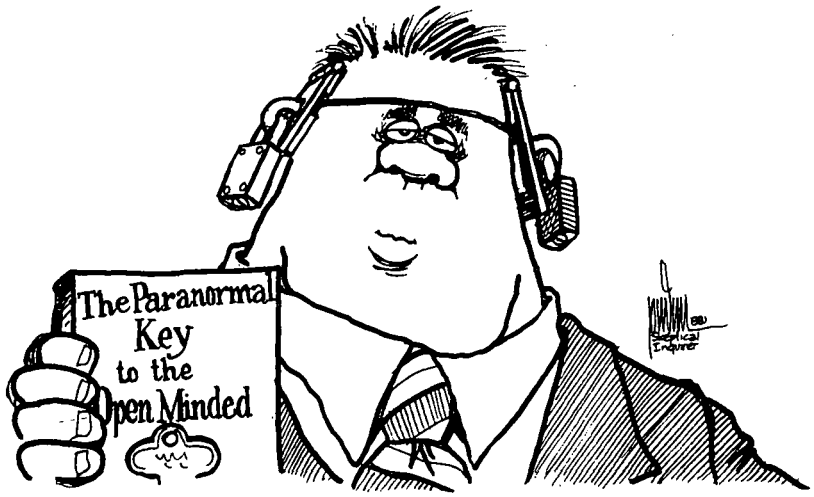
I

Even though charges of closed-mindedness are usually brought in as a shoddy rhetorical device to prop up an otherwise hopeless argument, there may sometimes be a serious, though muddled, point lurking somewhere behind such charges. Believers in the paranormal often point to the truism that it is impossible to prove the nonexistence of something,[1] as if this point counted in favor of their position. Of course it doesn't; it is just as helpful for demonstrating the existence of the Easter Bunny as it is for demonstrating the existence of spooks. Unfortunately, it rarely does the skeptic any good to point this out to a believer in the paranormal; more often than not the upshot is that the believer will add the Easter Bunny to his belief system rather than drop spooks. In any event, some believers in the paranormal may be making an appeal to the impossibility of proving nonexistence when they charge skeptics with closed-mindedness. Let us reconstruct the most plausible version of such an argument:

> It is impossible to prove the nonexistence of something; hence it is impossible to prove the nonexistence of paranormal phenomena. Closed-mindedness is believing in something in the absence of proof.[2] Therefore, those people who believe that paranormal phenomena do not exist are closed-minded.

There are a lot of problems with this argument. The most serious one, however, is that, if it were valid, another argument of the same form would show that believers in the paranormal are equally closed-minded. To see this, we need to remind ourselves of what we mean by "paranormal event." A paranormal event would be one that could not have been brought about by normal causes—we can leave aside for present purposes the question of what constitutes a normal event, because it doesn't affect the present argument. The important point is that, in order to have evidence that a given event is paranormal, we must first have evidence that there were no normal events present that could have caused it. We can now see that a defender of the paranormal who used the argument above would be hoisting himself by his own petard:

> It is impossible to prove the nonexistence of something; hence it is impossible to prove the nonexistence of a normal cause for any event.



But it is necessary to prove this in order to prove that a given event is paranormal. Hence it is impossible to prove that any events are paranormal. Closed-mindedness is believing in something in the absence of proof. Therefore, those people who believe that there are paranormal events are closed-minded.

Since defenders of the paranormal see themselves as paragons of open-mindedness, they should be careful about entering into an argument that invites us to draw the conclusion that they are closed-minded. In fact there are some defenders of paranormal claims who are genuinely open-minded; but these people spend their time examining the evidence for their claims rather than engaging in pointless disputes about whether others' minds are open or closed.

II

A related version of the "Argument from Closed-mindedness" takes the following form: The defender of paranormal beliefs expects to silence the skeptic with, "They laughed at . . .", where the blank can be filled in with the name of any respectable scientist (Copernicus, Galileo, Einstein, or some other). The conclusion we are expected to draw from this opening gambit is that, even though he may be "out of fashion" now, the crackpot that the believer in the paranormal is defending will be seen by future generations as a great scientist who saw the truth long before the rest of us closed-minded people did. Only open-minded people such as this particular crackpot, and of course the defender of the crackpot, can see the truth now.

Usually this argument goes wrong at the point where the paranormal defender says, "*They* laughed . . ." After all, who laughed at Einstein before examining and coming to understand his theories and the evidence for and against them? Only crackpots; not genuine scientists. The crackpots didn't like what they took the theory to be; and the only weapon open to them was laughter, since they didn't understand enough theoretical physics to attack it on its own grounds. The scientists didn't laugh; neither did they simply believe it on faith. They were too busy making sure that they understood what Einstein was offering for their consideration, and then devising experiments that would confirm or count against particular points in Einstein's speculations. In these endeavors, Einstein and the scientific community were working together, even though it may have appeared to nonscientists that they were at war with each other.

True, there have been some people, such as Velikovsky, who have been at war with the scientific community. But in Velikovsky's case, the war was declared by him, because he thought it was grossly unfair of the scientific community to treat him in the same way that genuine scientists *expect* to be treated. A genuine scientist expects that, if he makes a vague statement, a colleague will help him out by asking him to clarify it; he does not expect that his "genius" will be recognized *before* he presents his data. People like Velikovsky cannot have it both ways: they cannot reasonably expect to be considered genuine scientists just like everyone else in the scientific community and at the same time to be treated differently from everyone else. It is contradictory expectations such as these that identify someone as a *pseudo*scientist. Velikovsky's charges of closed-mindedness against the scientific community had about the same amount of evidence to back them up as his cosmological theories had—after all, he presented little, if any, hard data that could be tested. If he had presented such data, we would be in a position to judge how open-minded the scientific community was by seeing how they dealt with it. However, answering the question "If Velikovsky had presented hard data for his theories, would the scientific community have been open-minded about it?" is something like answering the question "If a curly tail were a leg, would a pig have five legs?"

Velikovsky's charges of closed-mindedness serve as a classic example of pseudoscientific question-begging. The unfairness or closed-mindedness of laughing at Einstein can be appreciated only if we recognize that his claims have a high probability of being true. Similarly, the unfairness of laughing at Velikovsky presupposes that his claims have a reasonable probability of being true, which was just the point in question in the Velikovsky business—or at least it ought to have been.

III

Why is it that, despite the fact that charges of closed-mindedness are red

herrings in these debates, they are trotted out with such regularity? I think that it may have something to do with one of the persistent themes to be found in many science-fiction stories. Science fiction has as its main virtue the capacity to open our intellects to new possibilities and new ways of thinking about things. Many science-fiction stories also preach to the reader about the importance of keeping an open mind. The hero of many science-fiction works is an inventive, creative genius who must overcome not only tremendous technological problems and an unforgiving nature but also a number of twits in high places with small and closed minds. The climax of the story is often a scene where the hero either thumbs his nose at these people or bows out with their unending adulation. Great stories, and a good moral. However, if people receive only this moral, without also learning how to distinguish reasonable from unreasonable beliefs, they are ripe for the paranormal explosion. Note that I am not claiming that science-fiction writers are responsible for the paranormal explosion. It is not the job of science-fiction writers to teach people how to distinguish fact from fiction—after all, science-fiction *is fiction*. However, people who read only these kinds of stories—or worse, science fiction plus other works of fiction not clearly labeled as such, like von Däniken's, have their minds open to receive a lot of junk.

One answer to this problem is to encourage people to read good popular works on science, rather than works on the Bermuda Triangle or on auras, in addition to science fiction. Another answer would be to have more science-fiction stories that dramatize the virtues of tough-mindedness as well as open-mindedness. But yet another answer is to make sure that, when we read a science-fiction story that depicts opposition to the ideas of the hero, we don't fall into the trap of seeing straightforward opposition to ideas as closed-mindedness. All stories are told from a *point of view*, usually that of the hero. If an author gets us to believe, or to hope, that the hero is on the right track about something in the story, then it is natural for us to view his opponents as being somewhat stupid or closed-minded because they don't see this too. Perhaps the author doesn't *intend* us to view the opponents this way, but it is often a natural consequence of our adopting the point of view of the hero rather than that of the opponents. A similar kind of think often happens to proponents of the paranormal: often these people have no training in the scientific method; they have no idea of how to weigh and assess evidence before believing a claim. They simply *adopt the point of view* of a Velikovsky or a Berlitz and see anyone who questions their hero as closed-minded.

H. G. Wells's "The Country of the Blind" is one example of a story told from a particular pont of view that may beguile an incautious reader into thinking that the opponents of the hero are closed-minded, even though the evidence presented in the story does not justify this attribution.[3] Although it is not of the science-fiction *genre*, it is worthy of comment.[4] In this story, a man with normal vision blunders into an isolated community

where many generations previously a disease had rendered the population congenitally blind. It has been so long since anyone has had vision that the vocabulary having to do with sight has been long forgotten, and all knowledge of the world that comes from sight has been lost. Of course our hero finds himself in a lot of trouble when he mentions things of which the people of this community are unaware and when he prattles on about an "extra" sense. He is, in fact, considered crazy.

One interpretation that could be drawn from this story is that if these people only would be less closed-minded and listen to this person who has a capacity that they lack (Uri Geller? Jeane Dixon?) they would come to understand the world more fully. Or, if one of their own people were open-minded enough to recognize the possibility of a new sense and would be willing to experiment on this stranger (Russell Targ? Andrija Puharich?), they could come to see the world aright.

But this story, upon closer examination, does not indicate closed-mindedness. These people do have some false beliefs, and they would be better off if they dropped them. But these beliefs are not so much the result of lacking vision as the result of mistaking inferences from the perception they have from their four working senses. Thus it doesn't require a person with an extra sense to help correct these; it requires better use of their reason.

There also are many beliefs that they do not hold but that people with vision normally do hold. But the fact that they do not immediately adopt the beliefs of the outsider with vision does not, by itself, suggest closed-mindedness. For there is a dilemma here: Either these beliefs can be had *only* by people with vision (e.g., arguably, the belief that physical objects have color); or there are ways open to the blind people to verify this information through the four senses that they do possess. The first option corresponds to what mystic proponents of the paranormal maintain is the case with "psychics." But, on this interpretation, such information could *never* by obtained by people who lack this sense. So in what sense are these people closed-minded for doubting the existence of that for which they couldn't possibly have evidence? And what would be the sense of wishing that they be open to that which they couldn't possibly get?

The second option in the dilemma above is that this information can be had or verified by someone who lacks this extra sense. But, if so, the blind people cannot be in a position to verify the outsider's information until they discover methods of verification open to them. Before they have carried out their experiments and devised their methods, they cannot be accused of closed-mindedness for not believing the claims of the outsider. For all they can be expected to know, the outsider could have one or two fewer sense(s) than Uri Geller, but all his *chutzpah.*

In Wells's story there are shortcuts to the truth, open to some people but not to others. Does this suggest the possibility that there may be other shortcuts to the truth of which we are not yet aware? Yes, it does. But we

must remember how much *less* is accomplished when someone *suggests* something than when he *provides evidence* for it. We might forget this point when we read Wells's story if we allow ourselves to be beguiled by the story and forget that he is telling the story to *us*, who already know perfectly well that there is such a thing as vision. The evidence that there is vision has *already* come to us from our *experience*, not from the fictional account Wells provides. So it is absurd for us to blame the people in the story for being closed-minded. For one thing, they lack the evidence we have. For another, there is not much point in blaming fictional characters for anything. So the believers in the paranormal might profitably ask themselves two questions before drawing consolation from Wells's story. First, what is *their* evidence that comes from experience prior to reading the fictional accounts contained in books on the paranormal? Second, what is the point of drawing analogies between closed-minded skeptics of the paranormal and the characters in Wells's story? Most of the former group are just as much creatures of the imagination as the latter.

Notes

1. Actually, we *can* conclusively prove the nonexistence of *some* things that people claim exist, namely, those things whose descriptions are self-contradictory—e.g., we can conclusively prove that round squares do not exist, because the term "round square" is a self-contradiction; thus there can be no object in the world that answers to it. Many people have tried to prove that certain paranormal phenomena do not exist by trying to find a self-contradiction in their description. But skeptics like those involved in CSICOP do not take this line for two reasons: (1) many descriptions of paranormal phenomena do *not* involve self-contradiction; the problem lies with the *evidence* for the claims; and (2) when descriptions *do* involve self-contradiction it is often possible to *remove* the contradiction, and then the description is a case like (1). Contrary to popular belief, skeptics are often extremely charitable, in that they spend a lot of time trying to make cases of (2) into cases of (1) before they can get down to the serious work of assessing the evidence for them.

2. It is at least this. It is also the refusal to change one's belief in the light of further evidence.

3. H. G. Wells, "The Country of the Blind," in *The Short Stories of H. G. Wells*, Doubleday, Garden City, N.Y., 1929, pp. 163-90.

4. For a more recent science-fiction story that can be misread in the same way, see Robert Heinlein, *Orphans of the Sky*, Berkley, New York, 1963. •

When once miracles are admitted, every scientific explanation is out of the question.

—Johannes Kepler (1571-1630)

Book Reviews

Parapsychology: Science or Magic? By James Alcock. Pergamon Press, Elmsford, N.Y., 1981. 224 pp. $17.95.

Reviewed by Andrew Neher

The most important thing to say about James Alcock's *Parapsychology: Science or Magic?* is that it's a dandy book. I know of no book that does a better job of presenting (1) the psychology of belief in psychic events, i.e., *why* many people are predisposed to accept such beliefs, and (2) the pitfalls of parapsychology as a science. As an added attraction, Alcock is lucid, thorough, well-organized, and does a fine job of citing the relevant literature.

In the first five chapters, Alcock shows:

How some beliefs in psychic ability are reminiscent of magical beliefs; for example, bringing harm to an enemy by using his nail clippings to cast a hex on him has its modern counterpart in the practice of psychometry, whereby a personal belonging is thought to contain an "essence" of its owner that can be sensed by a suitably psychic "reader."

How belief in psi might meet many of the same psychological needs that—according to various theorists—give rise to religious beliefs, e.g., needs to explain mysterious events, to deal with the unpleasant fact of death, to provide a sense of purpose, and to provide social cohesion.

How psychic beliefs often resemble superstitions that develop because they are reinforced—albeit in a capricious fashion—according to operant conditioning theory. For example, a baseball player who happens to play his best game of the season while carrying a lucky penny given him by a fan may develop a superstitious "faith" in the "power" of the penny.

How modern parapsychology emerged from a backdrop of such magical, religious, and superstitious beliefs, especially the popular nineteenth-century belief in spiritualism, whereby believers maintained that spirits manifested themselves, in various forms, in the physical world.

How interest in the psychic realm seems to be enjoying a resurgence; e.g., books listed under "Occult and Psychic" in *Books in Print* went from 131 in 1965 to

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1,071 in 1975, and the number of periodicals directed to such topics grew from 12 in 1968 to 46 in 1978. Reasons for this upsurge might range from disenchantment with the emphasis on rationality in contemporary society to the inability of traditional religious beliefs to meet desires for a scientific-*appearing* belief-system.

How belief in psi might stem from, for example, the tendency to explain a "mysterious" event by postulating a "mysterious" mechanism, and the tendency to accept the beliefs of those around us or—in the absence of a better alternative—the word of self-proclaimed "psychic" authorities. Whatever the source of such beliefs, people who hold them seem more emotionally committed to their beliefs than disbelievers are to their *dis*belief; studies have demonstrated that believers, more than skeptics, distort their experience in an effort to maintain their belief.

How certain psychological anomalies produce experiences that can *appear* mysterious or paranormal and thus be interpreted as such; e.g., subliminal perception, by means of which subtle cues are picked up and processed without conscious awareness; psychological set, whereby prior conditioning predisposes us to misinterpret events (witness magic shows and optical illusions); "gestalt" distortions, which twist our recall of events to better fit our biases; mystical experiences, the overpowering nature of which seems to argue for the existence of "another reality"; strong imagery, or hallucinations, which *seem* to be glimpses of "another world"; *déjà vu* experiences, which seem to demonstrate precognition; and automatism, which is involved in dowsing and ouija boards and which is sometimes misconstrued as "mind over matter," or psychokinesis.

How common errors of reasoning about causation and probability can lead to beliefs in "mysterious forces." For example, research indicates that subjects tend to see meaningful connections, particularly of a cause-effect nature, between events even when they don't exist. Thus, again, the baseball player who plays a great game on the same day he is given a lucky penny is likely to see a (cause-effect) connection between the two events.

In spite of the evidence that Alcock marshals to make his case that belief in psi is rooted in irrational human needs and aberrant thought patterns, a true believer could justifiably protest that, thus far, it is an 'ad hominem' argument that ignores the evidence for psi that parapsychological researchers have been accumulating over the years. Thus Alcock switches targets and takes aim, in Chapters 6 and 7, at the field of parapsychology, which, after all, claims to be a scientific discipline. In evaluating the claims of parapsychologists, he argues that:

Parapsychology conflicts with established sciences, which detracts from its credibility. In particular, although some "paraphysicists" feel that certain contemporary theories in physics (relativity and quantum mechanics) are congruent with psi, most noted physicists do not.

Parapsychologists who define *extra*sensory perception as *non*sensory have the impossible task of proving a negative; as Alcock says, "we can never prove that there are not sensory channels of which we are not aware." Many parapsychologists thus blind themselves to the possibility of discovering new and subtle forms of *sensory* communication.

Parapsychology today is no closer to "proving" psi than it was at its inception a hundred years ago. In particular, it has yet to come up with a truly repeatable experiment, which is the hallmark of an established science.

Parapsychologists, in an unscientific manner, tend to deal with negative results by failing to publish them, by claiming that psi can be "stifled" by skeptical experimenters, by claiming that psychic abilities fade with time (the "decline

effect"), or by accepting extremely negative results as proof of "psi in reverse" (so-called "psi-missing").

Parapsychology has too often been guilty of sloppy experimental conditions that go unreported, experimenter fraud (several cases of which have been reported recently), and failure to pursue systematic research that refines procedures in the light of accumulated knowledge.

Parapsychologists have sometimes erred in their use of probability theory. A common error is the failure to provide actual controlled conditions in experiments. Instead, parapsychologists commonly use *theoretical* estimates of outcomes to be expected if psi were not operating. The problem is that the "real-life" conditions of the experiment may produce significant departures from theoretical estimates even in the absence of psi.

Having thus concluded that the field of parapsychology has disregarded many crucial scientific maxims, Alcock devotes his remaining two chapters to:

An exposé of deceptive media practices that result in manufactured or inflated accounts of seeming psi experiences. The reason? That's what sells. The result? A public that is continually misled about the state of the evidence for psychic phenomena.

The issue of what the world would be like if psi existed. For example, scientific knowledge (including knowledge re psi!) would be impossible, since each researcher—through his psychic powers—would bias his results to fit his individual beliefs.

A plea for better education in the scientific method. Unfortunately, given the current drive to dismantle education, at least in the United States, this is not likely to happen in the foreseeable future.

By now it should be clear that Alcock is a skeptic regarding claims of psychic influences. However, the question arises: Is his case a valid one? In my opinion, there is little question that Alcock is correct about the psychological factors that predispose many people to believe in psi. However, his general condemnation of parapsychological research is another issue. From what I have seen, a typical study in parapsychology is not necessarily more flawed than a typical study in mainstream psychology. That such flaws are so relentlessly tracked down—in parapsychology—is due, I believe, to the a priori improbability of many of its findings, namely, those favorable to psi. This does not mean, of course, that parapsychology has more substance than it is usually given credit for; it means only that mainstream psychology has *less*. Of course Alcock might agree with this perspective, even though he is not explicit on the matter.

On the whole, then, I highly recommend Alcock's book. For the debunker, it provides ammunition. For the believer, it presents a well-defined position to marshal arguments against. (After all, in this yin-yang world, debunkers and believers *do need* each other.) ●

The Psychology of Transcendence. By Andrew Neher. Prentice-Hall, Englewood Cliffs, N.J., 1980, 361 pp. $7.95, paper.

Reviewed by James E. Alcock

This is a splendid book that deserves careful reading not only by those interested in claims of the paranormal, be they believer or skeptic, but by all who want to better understand the nature of consciousness. Neher's subject is transcendental experience, and he sets out to explain not only how such experiences may be generated by quite normal processes but also why people often feel the need to invoke paranormal/supernatural explanations for them. He succeeds admirably in both quests, and along the way he both educates and entertains, even so far as teaching the reader how to generate some of the experiences he describes.

Neher spends the first several chapters discussing psychological/physiological processes that might be expected to produce apparently supernormal experiences from time to time. Because people lack awareness of their natural basis, these fascinating effects are often ascribed to mysterious forces. His discussion opens the reader's eyes to an array of processes likely to be unfamiliar to the layperson, such as visual after-images, neural habituation and inhibition, sensory driving, perception without awareness, and so on. He describes how conditioning processes are important—how feelings of ecstasy can be elicited by stimuli of which we are not even aware—and how the effects of suggestion may facilitate either illness or healing. Water-witching, possession behavior, automatic-writing, and other related behaviors are discussed as examples of how lack of awareness of our own mental activity may lead us to believe that an experience is brought about by forces outside ourselves. The remainder of the book is devoted to a more direct analysis of transcendental experience itself, and Neher divides this analysis into three parts: the mystical, the psychic, and the occult. Under the rubric of mystical experience, Neher discusses the various conditions, such as fever, hyperventilation, drowsiness, and so forth, that seem to be favorable to the production of transcendental experiences, and he provides important insight into these experiences—for example, on out-of-body experiences: "When our sense of body position is no longer functioning, we can perceive our bodies as free-floating or our psyche as detached from our bodies" (p. 125).

His treatment of psychic experience includes a first-rate critique of the prophecies of Nostradamus and the abilities of Edgar Cayce. The information he provides is indispensable to the student of either of these subjects. He also analyzes psychic healing, Kirlian photography, auras, and other phenomena in this section.

In the section on occult experience, Neher analyzes, among other subjects, astrology, the Tarot, chiropractic, the Reichian orgone, Scientology, pyramidology, Von Däniken, UFOs, and the Bermuda Triangle. Critics often take the view that belief in the occult is harmful only to the extent that it weakens the propensity to seek rational explanation, in the same way that psychic surgery may be harmful only to the extent that its use delays people from seeking proper medical diagnosis and treatment. However, Neher argues that occult practices carry a great danger. He stresses that such beliefs are based on "powerful psychological needs

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and processes that operate outside of conscious awareness. Consequently, occult practices can have psychological effects that transcend ordinary experience." Occult explanations steer us away from understanding our own psychology, and he warns that "a naive view of these same transcendental experiences can produce a superficial occultism with potential dangers that arise whenever powerful psychological mechanisms are misinterpreted and misunderstood" (p. 209).

All through the book, Neher keeps reminding us that it is not the experience but the explanation that is given for it that needs to be carefully scrutinized and often challenged. His unique approach avoids a condescending and negative "you should know better" attitude toward such beliefs and instead provides a positive impetus to learn more about the way our bodies work. By understanding how physiological/psychological processes produce such experiences, we can come to enjoy the experiences, and even learn from the experiences (to the extent that they might be based on information absorbed without awareness, or on novel combinations of elements of experience, etc), without needing to fall into occultism with its attendant dangers.

The only reservation that this reviewer has, and it is a slight one, is that in bending over backwards to avoid the dogmatism that he ascribes to some debunkers, Neher has perhaps been less forceful than he might have been in cautioning people about putatively scientific evidence for the paranormal. For example, he states (p. 227), "At this point, therefore, the most reasonable conclusion seems to be that psychic phenomena have not yet been conclusively demonstrated." To my mind, even though it seems clear from the rest of what he says that Neher's views are more critical, this might be taken to suggest that the case for the paranormal is building but is not yet strong enough. The existence of quarks is accepted as a useful provisional hypothesis until such time as conclusive evidence is adduced. Can the psi-hypothesis be treated in the same way? I would think not. Similarly, when Neher argues that we should continue to search for the paranormal because eminent scientists (e.g., Freud, Jung, Alfred Russel Wallace, John Eccles, Sir William Crookes, and others) have believed in it or thought it likely, I fear that Neher risks teaching the reader to examine the claims for a phenomenon in terms of the eminence of its advocates. One could find scores of scientists, some eminent, who believe in the existence of racial differences in intelligence, but their *beliefs* should not spur us to seek such differences. Let the evidence speak for itself.

Apart from this minor criticism, which I believe stems from Neher's deep desire to be fair to all points of view and to avoid dogmatism, I have only praise for what is written. No one should dare to tackle the domain of paranormal experience without a solid grounding in the normal bases of experience. This book provides an excellent foundation. Bravo! ●

Note: Neither Alcock nor Neher knew that the other was reviewing his own book.—ED.

The Disappearance of Flight 19. By Larry Kusche. Harper & Row, New York, 1980, 205 pp., $12.95.

Reviewed by Michael R. Dennett

For those readers not well acquainted with the legend of the Bermuda Triangle, the disappearance of Flight 19 is probably its most celebrated "mystery." According to Adi-Kent Jeffrey in her best-seller, *The Bermuda Triangle,* the flight leader is heard to say in a frantic radio message: "Calling tower ... This is an emergency ... We seem to be off course ... We cannot see land ... Repeat ... We cannot see land...." Alan and Sally Landsburg report in their book *In Search of Ancient Mysteries* (which incidentally was featured as a Timex Special on NBC-TV) that a ham operator heard Lieutenant Taylor say, "They look like they're from outer space—don't come after me."

This episode has long awaited a comprehensive and penetrating examination. Kusche's book is just such a review. Meticulously researched over a period of seven years, it is exhaustive in its attention to detail and scrupulous in the presentation of the facts.

The Landsburg and Jeffrey accounts, as it turns out, are not even remotely associated with the actual events, but surprisingly Kusche has not written a debunking book. *The Disappearance of Flight 19* is rather a history of a small incident in American folklore. However, just because there are no magical forces at work between Miami and Bermuda, it does not mean that there is no story to be told.

It is, as the book's jacket describes, a tale of "danger, of leadership and responsibility, of miscalculations, and of the cruelest of ironies." Until now, the key figure, flight leader Charles Taylor, has been an unknown entity. Kusche brings the personality of Lieutenant Taylor out of the darkness. The path to reconstructing this man from almost forty years of distance is not easy, and our view of Taylor, to be sure, is limited. But, with the skill of an accomplished historian, the author makes the best of each scrap of information to assemble a believable portrait of Taylor.

The narrative is quickly paced and, although it moves from Taylor's Texas home to the wartime Pacific and from the deck of an aircraft carrier to an Avenger cockpit, the central theme is never lost. The question of what happened to those fourteen men is answered.

This work is investigative history at its best and could be used as a textbook for graduate students. In the presentation of the facts this most spectacular of "mysteries" is unavoidably demolished. Kusche makes no apology for the ravaging of this legend to the point of suggesting that the reader make his own inquiries. (If you are so inclined the transcript, about 500 pages, of the "Board of Investigation into Five Missing TBM Airplanes and One PBM Airplane" is available in microfilm from the Naval History Center in Washington and it is a good place to start.)

The Disappearance of Flight 19 is probably the best work on the "Devil's Triangle" to date, and is without a doubt the definitive account of the loss of

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Michael Dennett wrote "Bermuda Triangle 1981 Style" in our Fall 1981 issue.

Lieutenant Taylor's flight. For anyone interested in the Bermuda Triangle, regardless of your inclination, this book is mandatory reading. •

Psycles. By Dwight H. Bulkley. Bobbs-Merrill, Indianapolis, 1980. 340 pp. $10.95.

Reviewed by Terence M. Hines

And you thought biorhythm theory was strange! In *Psycles,* Dwight Bulkley presents another sinusodial rhythm-based theory of various human miseries. But the rhythms here are not days long, they are hours long. In fact, there is only one rhythm, the circadian cycle, with a period of 24.5 hours. But 24.5 is not the magic number here; 37 is. According to Bulkley, everything from accidents and psychological problems to epileptic seizures can be traced to events that took place 37 hours previously. Thirty-seven is almost 1.5 times the basic circadian period. Unfortunately, no attempt is ever made to explain why events that took place one and a half cycles previously, as opposed to those that took place one-half, one, two, or two and a half cycles previously, have so much influence.

And what evidence is there for this theory? Most of the book is taken up by short case histories that are alleged to show the importance and reality of the 37-hour period. For example, a man's hand was badly burned while he was helping the driver of a stalled car. Accident? No indeed! Thirty-seven hours previously, the fellow had used the injured hand to severely spank his infant daughter, and he felt guilty about it. Thus, the burn was self-inflicted, albeit unconsciously, as a punishment of the offending hand. The time of the "accident" was determined, for totally unspecified reasons, by the mystical 37-hour period. Incident after such incident is reported, but of course no mention is ever made of the myriad accidents that occur when nothing of import had taken place 37 hours previously. Thus Bulkley falls into the same trap as do biorhythm theory proponents: selectively picking only events that support their preconceived beliefs, while ignoring any conflicting data.

The example above points out Bulkley's highly symbolic view of human behavior. For him, there are no accidents. Accidents are just events caused by some trigger event that took place 37 hours before. The type of accident is determined by the trigger event and the symbolic nature of the human subconscious, the accident being related to the trigger event in some symbolic way. Of course, no evidence other than post-hoc rationalization is ever presented to show the validity of reality of such hypothetical symbolic relationships.

Even death, according to Bulkley, occurs in response to trigger events that took place 37 hours previously. For example: "In an industrial 'accident,' 9 A.M., Monday, August 24, 1959, Mike Sanger severed his jugular vein and bled to death.

Terence Hines, who wrote "Biorhythm Theory: A Critical Review" in our Summer 1979 issue, is now a member of the Psychology Department of Pace University's Westchester, New York, campus.

Saturday evening, about 36-37 hours before, his cloest friend, John Jacoby, was killed in an auto wreck" (p. 155). Sanger's death was presumably a suicide brought on unconsciously by the loss of his friend. The importance of trigger events, the unconscious, and the 37-hour period in determining death is not limited to the deaths of adults. In a most macabre section the author deals with his theory of sudden infant death syndrome (SIDS or crib death), which mysteriously kills thousands of infants each year. These deaths, we are told, are not due to any pathological biological processes. Rather, they are suicides (!) in response to a trigger stimulus, such as rejection, that occurred 37 hours before. Thus "it is quite simple: SIDS babies should be blessed as little martyrs who did the right thing, gracefully stepping aside and bowing out of a stressful situation" (p. 157).

And on and on: The victims of disasters, natural or otherwise, are seen as responsible for their death or injury. Take the sinking of the *Titanic:* "As I see it, the whole cooperative effort to sink the *Titanic* was required socially and engineered with precision (subconsciously) to dramatize the fallibility of Science . . ." (p. 201). People killed or injured in disasters are said to somehow psychically sense that these events will take place and, for various reasons, to move to the scene. Thus victims of sniping attacks, such as the one perpetrated by Charles Whitman in Austin, Texas, in August 1966, may not be just innocent victims. Rather, they may by unconscious accomplices. After all, as Bulkley points out, wasn't one of Whitman's victims a "newsboy on a bicycle who took a bullet in the buttocks" (p. 204)? What more clear-cut proof can there be that something symbolic is going on?

The book contains a chapter on how to improve your life and health through the use of and the knowledge of "psycles" and the standard attack on scientific medicine. Also included are several bizarre appendices, taking up 70 pages, on such topics as "Embryology of the Spinal Cord" and "A Revolution in the Secret of Life," the latter being an incoherent attack on modern biochemistry.

This book, then, is a mismash of various psychic beliefs tossed together to support the supposed importance of the 37-hour period. It is poorly written, often incoherent, and internally inconsistent. It is a masterpiece of pure crackpottery and one of the silliest books in the paranormal field that I've seen in the past several years. •

Some Recent Books

Listing here does not preclude a more detailed review in a future issue.—ED.

Herbert, Victor, M.D., J.D. *Nutrition Cultism: Facts and Fictions.* George F. Stickley Co., 210 W. Washington Square, Philadelphia 19106. (Distributed to bookstores by Scribner's.) 1981, 234 pp., $12.95. Noted physician and nutrition researcher provides straight, no-nonsense presentation on nutrition cultism and quackery.

Herbert, Victor, M.D., J.D., and Stephen Barrett, M.D. *Vitamins and Health Foods: The Great American Hustle.* George F. Stickley Co., 210 W. Washington Square, Philadelphia 19106. (Distributed to bookstores by

Scribner's.) 1981, 189 pp., $11.95. A strong investigative report on such subjects as the modern food quack, dubious doctoring, the "natural-organic" rip-off, prominent promoters, the laetrile story, nutrition and the media, and where to get accurate information.

Leith, Harry. *The Contrasts and Similarities Among Science, Pseudoscience, the Occult, and Religion,* 3rd ed. (Order from the author, Dept. of Natural Sciences, Atkinson College, York University, Toronto, Canada.) 1982, 105 pp., $2.75, paper. Updated, expanded (44 additional pages) edition of Prof. Leith's earlier (1978) bibliography. Hundreds of books and articles listed in 29 categories, such as Astrology; Healing; Glossolalia (Tongue Speaking); Parapsychology; Pseudoscience, Magic and the Occult; Survival After Death and Reincarnation; and UFOs and Space Visitors.

Radner, Daisie, and Michael Radner. *Science and Unreason.* Wadsworth Publishing Co., Belmont, Calif. 94002. 1982, 110 pp., $6.95, paper. Thoughtful, succinct, clearly written guide to pseudoscience and how it differs from genuine science. The authors focus on patterns of reasoning and philosophical assumptions of pseudoscience rather than analysis of specific claims. Abundant examples enliven the commentary. Among the eight chapters are "A Walk Along the Fringe of Science," "Crank or Revolutionary?" "Marks of Pseudoscience," and "The Mystery of Science." Recommended for its clarity, brevity, attractiveness, and intelligence.

—*Kendrick Frazier*

Articles of Note

Cherfas, Jeremy. "No Better Than Chance." *New Scientist,* Oct. 22, 1981. Interesting little article on a controlled, double-blind test that refuted a dowser's claim that he could detect voltages in an overhead wire by dowsing, and the issue of how a magazine should handle a refutation of a claim it previously reported.

Cole, John R. "Misquoted Scientists Respond." *Creation/Evolution,* Fall 1981. Anthropologist compiles responses of leading evolutionists to creationists' misquotations and quotations out of context from their works. The same issue contains "An Analysis of the Creationist Film *Footprints in Stone*" by Laurie Godfrey; and "Paluxy Man—The Creationist Piltdown" by Chris Weber.

Emery, C. Eugene, Jr. "For Tabloid Seers, 1981 Was the Year That Wasn't." *Providence Sunday Journal Magazine,* Jan. 3, 1982, pp. 14-15. Reporter chronicles psychics' failed predictions. Among the amusing ones: "Explorers in a South American jungle will uncover an amazing pyramid made of material not found on earth," investigators will "discover Sicily's Mount Etna is UFO base," and John Travolta will bring peace to the Middle East.

Gardner, Martin. "According to Hoyle." *Discover,* March 1982, p. 12. Describes

and criticizes Fred Hoyle's new theory, espoused most recently in his *Evolution from Space,* that natural laws cannot account for life's origin.

Gould, Stephen Jay. "Moon, Mann, and Otto." *Natural History,* March 1982, pp. 4-10. Another of Gould's superb columns dealing with evolution and creationism. This one begins and ends with his eyewitness account of scenes at the Arkansas creation-science trial, and in between muses over the dilution of evolutionary teachings from a widely used biology text and its out-of-context distortion of a famous, beautiful, "and courageous" statement by T. H. Huxley. There is also a moving passage about an Arkansas science teacher's dignified resistance to the creationist law, should it have been upheld.

Hofstadter, Douglas R. "About Two Kinds of Inquiry: 'National Enquirer' and 'The Skeptical Inquirer.'" (Metamagical Themas column). *Scientific American,* February 1982, pp. 18-26. Both an account of the work of the SKEPTICAL INQUIRER in combating nonsense "by recourse to common sense" (with extensive excerpts from some of the author's favorite articles) and an inquiry into the paradoxes involved in evaluation of exotic ideas. Amidst "the vast ocean of irrationality that all of us are surrounded by," says Hofstadter, the SKEPTICAL INQUIRER is "a steady buoy to which one can cling in that tumultuous sea."

Holden, Constance. "Cousins' Account of Self-Cure Rapped." *Science* 214: 892, Nov. 20, 1981. Report on criticisms of Norman Cousins's claim of self-cure of a serious illness related in his best-seller *Anatomy of an Illness.*

Kelly, Helen E. "In a Plain Brown Wrapper—Quackery by Mail." *ACSH News & Views,* November/December 1981, pp. 1ff (American Council on Science and Health, 1995 Broadway, N.Y., N.Y. 10023). Investigation of misleading or false mail-order health-product claims (breast enlargers, weight reducers, anti-baldness potions, etc). With a sidebar article, "The Role of Magazines in Mail-Order Health Fraud."

Kurtz, Paul. "Mars Effect" (Letter to Editor). *New Scientist,* Feb. 11, 1982, pp. 395-96. Response pointing up "several errors in fact" in an earlier letter by Michel Gauquelin (Jan. 7, 1982, p. 40) about the Zelen test and the U.S. test of the so-called Mars effect.

"License to Misinform." *Discover,* April 1982, p. 12. Blistering critique of the "In Search Of" television series hosted by Leonard Nimoy. "'In Search Of' deals with ['great mysteries'] in a pretentious, semi-documentary, and fraudulent way, blatantly distorting or ignoring facts. The unwary viewer is led to believe that the worst sort of hokum has been verified by reputable scientists or expert witnessess . . . one of television's shoddiest shows."

McKown, Delos B. "Creation Science: Newspeak Before Its Time." *ETC.: A Review of General Semantics* 38: 362-68, Winter 1981. Auburn University philosopher writes of similarities between the so-called "scientific creationists" (whom he lumps with members of the Moral Majority and other evangelists under the term the "Powerful Parson") and the Big Brother of Orwell's *1984.* "Each has a totalitarian twinkle in its eye, each knows what is really good for the people, and each uses language deceptively to further his ends."

Mechler, Gary. "Starry-Eyed and Blissfully Ignorant," *Los Angeles Times,* Jan 1, 1982, Op-Ed Page. Astronomer writes about "two important and disturbing phenomena": the rise of irrationalism (such as creationism and fears of a "Jupiter-effect" earthquake), and the decline in science and math education,

"which feeds on irrationalism."

Morris, Scot. "Martin Gardner" (Interview). *Omni*, January 1982, pp. 66ff. Interview ranging over Gardner's wide interests, from recreational mathematics to philosophy to the eccentrics and frauds of pseudoscience.

Oberg, James. "Close Encounters of a Fabricated Kind." *New Scientist*, Dec. 24-31, 1981, pp. 896-98. Follow-up on the Petrozavodsk "jellyfish UFO" incident, known in the West to be caused by launch of the Soviet Cosmos 955 spy satellite (*SI*, Fall/Winter 1977). Since the Soviet government still does not acknowledge the existence of the secret base from which the Cosmos was launched—nor does it officially believe in UFOs—the explanations offered in Russia have been amusingly contorted.

Overton, William R. "Creationism in Schools: The Decision in McLean vs. the Arkansas Board of Education." *Science* 215: 934-43, Feb. 19, 1982. Full text of Judge Overton's Jan. 5 opinion and ruling striking down the Arkansas creation-science-in-schools law.

Randi, James. "China's Armpit Savants." *Omni*, February 1982, p. 96. Item notes that some Chinese scientists have been bewildered by children who apparently can "see" with their armpits. Randi reports that this is a centuries-old magician's trick and tells how it is done.

Saklofske, D.H., I.W. Kelly, and D.W. McKerracher. "An Empirical Study of Personality and Astrological Factors." *Journal of Psychology* 11: 275-80, 1982. An examination of certain hypothesized relationships between zodiac signs and personality among 214 students finds none. "There were no significant differences between subjects classified according to odd vs. even sign and the personality dimensions of extraversion, neuroticism, and psychoticism. Neuroticism scores were not significantly different between subjects classified according to water and nonwater signs."

Smith, T. Scott. "Astrology Revisited—Review of Recent Books," *Archaeoastronomy*, vol. 4, no. 3, 1981, pp. 37-39. Astronomer reviews books on astrology by Culver and Ianna (very favorably) and Jerome and Gauquelin (not so favorably).

Thomsen, Dietrick E. "How Do You Spell the End of the World? S-Y-Z-Y-G-Y." *Science News* 121: 142, Feb. 27, 1982. Brief piece on the persistent popular belief in the "Jupiter effect," "refutations and partial retractions notwithstanding." Of the supposed fearsome astronomical alignment: "To recapitulate: IT WON'T HAPPEN. There will be no syzygy."

Walsh, John E., and David Allen. "Testing the Farmer's Almanac." *Weatherwise* 34 (5): 212-15, October 1981. University of Illinois research meteorologists put the long-range weather forecasts of the venerable almanac to the test of systematic evaluation of accuracy over the period 1975-1980 and find them not significantly more accurate than chance guessing. For instance, 50.7 percent of the monthly temperature forecasts and 51.9 percent of the monthly precipitation forecasts correctly predicted whether these conditions would be above or below normal. Chance guessing would give 50 percent correct. See also news summary in *Science* 215: 954, Feb. 19, 1982.

Zuidema, Henry P. "Less Evolution, More Creationism in Textbooks." *Educational Leadership* 39: 217-18, December 1981. How publishers, engaged in their own struggle for survival, are catering to creationists.

—*Kendrick Frazier*

Follow-up

Is ESP's Logic Ill?

*Philosopher Sidney Gendin's article "ESP: A Conceptual Analysis" (*SI, *Summer 1981) has provoked an unusual amount of interest and controversy. Our Winter 1981-82 issue carried four letters commenting on it and a reply from Dr. Gendin. Here is another commentary on the issues he raised by a philosopher and a respected parapsychologist.*—ED.

In exploring the conceptual boundaries of scientific inquiry, philosophers are always in danger of advancing a priori arguments that something or another is impossible, only to be contradicted by experimental investigation, often in an embarrassingly short time. Thus Hegel produced a "demonstration" that the planets are necessarily seven in number, only to have Leverrier and Adams provide a road map for the discovery of the eighth. On the other side of the ideological fence, Auguste Comte declared that some factual matters were forever beyond the determination of science and gave as a paradigmatic illustration the impossibility of determining the chemical constitution of the stars. Even as Comte's pen touched the paper, Fraunhofer was busy perfecting the techniques of stellar spectroscopy. Philosophers such as Hegel and Comte trap themselves both because they inadequately evaluate their assumptions and because they fail to familiarize themselves sufficiently with the methods and data of the areas they so readily discuss.

In his article, "ESP: A Conceptual Analysis," Sidney Gendin's foot often slips into the Hegel-Comte pit. In what follows, we shall try to show where these slippages occur, and, in so doing, suggest why we think that it would be premature to regard psi-research as hopeless.

Gendin begins by remarking that, though it may be more interesting than ESP laboratory research, he will not discuss the anecdotal material because it defies scientific appraisal. Having apparently decided that such material is scientifically valueless, it is then easy for him to show that naked statistically significant results no more support a hypothesis of psychic powers by living human beings than they support a hypothesis of diddling by leprechauns.

Gendin is surely correct on two points: if "psi" is meant simply as a label for "whatever it is that is going on" it cannot serve as an *explanation* of what is going on. And he is right to maintain that statistics alone, no matter how impressive, cannot pick out one hypothesis from a reservoir of infinitely many rivals and enshrine it above the rest. For a statistical test to count for (or against) a hypothesis,

that hypothesis must have some degree of plausibility. Such plausibility need not be initally very strong, but we must have some reason for taking the hypothesis with some degree of seriousness.

What might that reason be? Plainly enough, it originally lies in the anecdotal material which Gendin too casually dismisses. Of course one does not "have some peculiar competence to investigate anecdotes after the fact with the alleged aim of confirming or disconfirming them" just by virtue of being a scientist. Indeed, a great deal of harm has been done by well-meaning people who have supposed that their being good scientists in, say, physics gave them special competence to investigate human behavior. But, for all that, there certainly are rational means for doing quite a lot of sorting out—mostly negative—after the fact.[1] A good recent example of how one can do this is to be found in Allan Hendry's *The UFO Handbook*. Despite their deficiencies, courts of law have some effective techniques for learning something from human testimony. In anecdotal psi material, careful inquiry leaves one with a relatively small number of cases (rarely involving psychic "stars") for which "normal" explanations are hard to find. More important, some of these cases that suggest extraordinary means of communication also suggest what kinds of observable or controllable variables may affect the performance in question. These, we would suggest, invite further investigations along particular, determinate lines. Critical examination of the first, anecdotal stage should generate specific, *refutable* conjectures, which are then tested in the second, laboratory stage. To say that carefully sifted anecdotal evidence confers plausibility upon a hypothesis is not to say that it makes that hypothesis credible. Properly conducted laboaratory work is required for that.

Very well, but what is this hypothesis? What degree of scoring success ought it to yield? In the absence of a theory of psi-phenomena, what grounds do we have for preferring it to the hypothesis that the results are obtained by long lucky runs?

What is to be tested is, of course, a family of hypotheses. Their general form is that there exists one or more communication channels not hitherto understood and that information is transmitted and received along these channels. What is to be investigated is whether there are signals that cannot be accounted for by currently acknowledged sources and on what variables the signals depend. A full-fledged theory of the mechanisms whereby such signals, should they exist, are generated, transmitted, and detected is not required, although it would certainly be helpful. In the absence of a theory, one might hope to discover and explore some of the relevant parameters of the process with a view to producing stronger signals, improving the reliability of their transmission, and establishing the constraints that an acceptable theory of the process must satisfy.

It should be noted that throughout his piece Gendin never does comment on how one should design and conduct experiments to evaluate the potential existence of a channel of information transfer not currently understood. In a well-conducted ESP experiment, one designates a source and a receiver, erects barriers to currently understood means of information transfer from source to receiver, systematically varies the message at the source in accordance with an algorithm incorporating an adequate random component, monitors the behavior of the receiver, and evaluates the extent of correspondence between the message at the source and the receiver's behavior. How would Dr. Gendin do it?

Our use of the jargon of information theory is deliberate, for the problems involved have a great deal in common with those of engineers who busy themselves

with extracting signals from noisy channels. They can often achieve quite remarkable results with very scant prior knowledge of the character of the signal with which they must deal. The more both ends of the transmission chain can be manipulated, the more information can be transmitted. Most human capabilities can be enhanced by training. It is reasonable to suppose that, if there are previously unsuspected information channels in human beings, the use of these channels could likewise be enhanced by careful training through feedback. In fact, it seems that if psi research is ever to attain more satisfactory levels it *must* move in these directions.

If we are to do without the language of the communications engineers in discussing these matters, it is natural to fall back on the vocabulary of perception. Here Professor Gendin greets us with a battery of arguments with the same conclusion. "ESP cannot be regarded as a form of perception." This is because (1) perceptions, unlike "psi-missing," are paradigmatically sources of correct information and thus "do not work by leading us consistently to error"; (2) perceptions are the means by which we confirm beliefs, "but there are no phenomena for which an ESP experience is the ultimate court of appeal"; (3) "normal perceptions . . .typically yield knowledge" but "in multiple target-guessing experiments . . . good subjects generally go wrong far more often than they go right."

Professor Gendin needs to familiarize himself more with the literature on sensation and perception. Perceptions need not be sources of correct information and frequently lead us into error. To take some familiar examples, light-rays are bent upon traversing an air-water interface. We are thus consistently misled as to the position of objects under water, unless we take the time to learn to correct for this consistent error. Those who are color-blind are consistently misled by certain of their color perceptions and cannot easily learn to compensate. A person staring at a dot of light in an otherwise darkened room will consistently report small movements of the light, even though the light is in fact completely still.

In similar fashion, the data from studies of subliminal perception and perceptual defense[2] provide abundant evidence that stimulation from the environment can have statistically detectable effects upon human behavior at relatively low levels of accuracy and with the person reporting no awareness that any information was processed. Consistent, systematic errors in signal transmission and decoding as well as the vagaries of information transfer under conditions of a low signal to noise ratio and low reliability have been extensively investigated and must regularly be taken into account by sensory physiologists, cognitive psychologists, and communications engineers.

A further word about Gendin's remark that "there are no phenomena for which an ESP experience is the ultimate court of appeal." Doubtless this is correct, but whether it will be so in the future is surely not now decidable. *If* there is, in fact, an information channel that is responsible for some of what people now call "ESP experiences," and *if* it should prove possible to train talented subjects in its use so as to make it highly reliable in specifiable circumstances, it might be possible to use it as a full-fledged scientific instrument in cases in which it would be difficult, dangerous, or impossible to obtain the results in any other way. It might be used, say, to discover whether other galaxies have inhabited planets, or to examine the innards of living organisms, or to settle historical questions. Of course one would have to test the reliability of ESP by means of the other senses, but we might then have good inductive reasons for using it in circumstances in which those fail us. The

procedure would be no different from the extension of instrumentation in other scientific fields. This imaginative example (and it is no more than that) presupposes that we would have a tolerably adequate theory or reliable technology of ESP, but it does show that it is *conceivable* that ESP could, in some instances, serve as "an ultimate court of appeal." Thus Gendin's reasons for asserting that ESP is not a form of perception do not seem persuasive. Of course ESP does not have to be true perception for ESP research to be valid; ESP research is merely looking for descriptive evidence of some form or forms of information transmission systems and their properties.

It is when he discusses the conceptual possibility of survival, precognition, and PK that Professor Gendin comes closest to falling into the Hegel-Comte trap. We have nothing to say here about his remarks concerning the concepts of survival and precognition beyond two precautionary observations. The first is that philosophers are by no means unanimous in finding conceptual impossibilities just where Gendin finds them. Can effects precede their causes? Is time-travel possible? Can disembodied minds exist? Competent academic philosophers have divided on these issues. This may only show, as it has often been remarked, that there is no opinion so strange that it has not been held by some philosopher or other, but it behooves us to be modest lest the egg appear on our own faces. The second precautionary remark is that the dividing line between the conceptual and the empirical has come to seem increasingly fluid during the twentieth century. Things that seemed conceptually impossible turned out to be impossible only within certain intellectual frameworks, and so, under empirical pressure, we found it within our power to devise new ways of thinking. Thus we now live comfortably with the relativity of simultaneity and spacetime (curved spacetime at that) and there are those who think that quantum mechanics forces us to alter elementary logic. Again, we do not wish to press the point too far. If there be genuine psi phenomena, they may turn out to be accommodatable within our present physical framework, or, on the other hand, they may cause us to flirt with making the most drastic conceptual changes. The fact is that we just don't know and are in no way obliged to cross any of these bridges until we know something more about the logical contours of the phenomena. On the other hand, we can agree with Gendin that no one is justified in tossing around notions like "precognition" and "disembodied existence" as if they were as unproblematic as "table" and "star."

What strikes one about Gendin's argument that PK is "wildly implausible" is that a precisely parallel argument could be advanced to show that nobody could, by just concentrating on the task, lower his blood pressure or his pulse rate: For in order to try one must know what constitutes a try. A try is not a mere tensing of muscles, a screwing-up of the brow, or a focusing of attention on some desired result. Try, for instance, to will your blood pressure to lower. Now try willing it to rise. What did you do differently? Why would someone regard thinking of his blood pressure and saying, "Lower, blood pressure, lower!" as constituting a try, even if only an exceedingly poor one? To believe you are trying is no guarantee that you are trying. Or, at best, mere belief that you are trying guarantees trying in the most jejune sense of the term; so there is no relationship between believing you are trying and succeeding. Consider someone who tries to lower his blood pressure by trying to will that a lamp stay lit. Is this more ludicrous than saying, "Lower, blood pressure, lower!"? Neither "try" is part of a rational plan except by luck (unless the act is a particularly simple one).

How does one in fact learn to lower one's blood pressure? By willing that a lamp stay lit! By Gendin's understanding of the logic of voluntary action, this ought to be a perfectly absurd task. Still worse, Gendin tells us that "one can't bring about an outcome without bringing about the means to that outcome." And, apparently, one must separately will the means. "I take it that it is an indisputable fact that those who will the outcome of dice do not also bother to will the movements of the dice. One can take the matter further and say that nobody knows even how to try to will the movements of the dice." By the same reasoning, to lift my finger I must first cause the appropriate neurons to fire. But I don't bother to will them to fire nor do I even know how to try to will them to fire. Except—by raising my finger! We suggest that Professor Gendin first get straight how it is that he can raise his finger and lower his blood pressure. We will then be pleased to hear his critical comments on such topics as using biofeedback to train people to alter the output of random-number generators.

Finally, we confess our perplexity with the advice Gendin gives to all of us concerning experimental precautions: "*Any* experiment is unsatisfactory if it is agreed to beforehand by the subject, for the experiment then becomes *his*, not the investigator's." If we are to take him at his word, then we would be forced to experiment with human subjects without their informed consent. We suspect that what he intends to say is that the experimenter rather than the subject should design the experiment and be responsible for seeing that its protocols are observed. Of course.

The last piece of Gendinian advice is that experimenters attempt to "bypass the need for statistical analysis" by producing a "decisive experiment." Ought we also bypass the need for science by receiving all Truth from the Holy Ghost? Decisive experiments are hard enough to come by in the physical sciences and ever so much more difficult in the human sciences. Until the millennium arrives, we think it worth laboring to achieve the more humble goal of *replicable* results.

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Notes

1. For an example, cf. Morris's analysis of the Amityville Horror in *SI*, Spring/Summer 1978. A court reached essentially the same conclusion (*SI*, Winter 1979-80). For a survey of investigative techniques, see Morris, "A Survey of Methods and Issues in ESP Research" in *Advances in Parapsychology*, vol. 2, Stanley Krippner (ed.), Plenum Press, 1978.

2. N. S. Dixon, *Subliminal Perception: The Nature of a Controversy*, McGraw-Hill, New York, 1971, for an overview of the methods and findings in this area.

Rejoinder to Hardin and Morris

What are anecdotal materials anecdotes of? Suppose John Doe reports something strange has happened. But what? John Doe interprets it as an instance of ESP but has he reason not to attribute it to something else, say the "diddling of leprechauns"? Hardin and Morris accuse me of too casually dismissing anecdotes, but they have begged the question by assuming that the anecdotes, if true accounts of strange events, *need to be* interpreted as ESP phenomena. Why else would they so casually assume that ESP is a more plausible explanation of strange events than rival explanations?

I gather from their talk of "communication channels" and "information theory" that Hardin and Morris have made up their minds that the "extra" in "extrasensory" refers to some as yet unknown perceptual apparatus and that it does not refer to some nonsensory ability beyond the perceptual. This places them squarely outside the main tradition, which includes those such as Rhine and the founders of the British Psychical Society. This may be all to the good, but it seems to me to be premature and hence closed-minded. Those who believe that ESP defies normal explanations should be wary of having Hardin and Morris as allies.

My two critics are unhappy about the fact that I do not explain how to design and conduct experiments that would evaluate the potential existence of a "channel of information transfer." Why should I? It was my purpose to show some conceptual difficulties with ESP, not merely empirical ones. So the complaint boils down to the fact that I did not write the paper my critics would have preferred me to write.

Hardin and Morris engage in the slightly abusive game of suggesting I familiarize myself with "the literature on sensation and perception." They offer this kind suggestion in the course of pointing out that perceptions are often erroneous. The examples by which they prove this point are well known and, as Hardin surely realizes, are used by every philosophy teacher who ever taught Berkeley's theories in an introductory course. In any case, of course everyone knows that perceptions are sometimes erroneous. How do we determine that? Don't we use more perceptions to discover that? Hardin and Morris claim that once we have tested "the reliability of ESP by means of the other senses" then ESP would do fine as an ultimate court of appeal. (Please note that they refer to the senses as the *other* senses, thereby implying that ESP is a sixth sense.) But if ESP is a genuine sensory ability why should we establish its credentials by reference to other senses? Do we verify that a rose smells sweet by touching it or that a rose is red by smelling it? The fact is that so-called ESP takes many forms—it sometimes overlaps with one sense organ and sometimes with another. It is not the name for a unique "information channel" and persons who are "good" at it under experimental conditions report no distinctive perceptual experience when they call out what later prove to be correct hits. Sorry. Professors Hardin and Morris, the sheer blurting out that something is the case can never be the ultimate court of appeal that it is the case.

My critics think my worst error concerns my remarks on survival, precognition, and such. Here I come closest to falling into what they label the "Hegel-Comte" trap. Yet, although this is my worst error, Hardin and Morris wish only to make precautionary observations and do not make so bold as to criticize my points. The first precautionary remark is that plenty of philosophers disagree about what is and what is not conceptually impossible. What of it? Did I ever suggest otherwise?

But how do philosophers reconcile their differences if not by doing more philosophy? If A thinks B is wrong then he does just what I said he must do: try to find a mistake in B's analysis. You don't prove a position is respectable by finding someone who holds it. One of the fundamental errors of parapsychologists is to think otherwise. The second precautionary remark is that sometimes things seem "to be impossible only within certain intellectual frameworks." Now appeal to "intellectual frameworks" is a wonderful old chestnut to fall back upon when you have no specific criticism to make. The expression is bandied about all too loosely. What is the intellectual framework that makes the idea of a 5-sided rectangle impossible? Are there alternative frameworks in which it is possible? Now it may be that *some* claims that certain things are impossible do turn on the acceptance of a particular framework, the acceptance of which is challengeable. But that ought to be argued for in each particular case. Mere sloganizing about "intellectual frameworks" is a high-minded way of setting up a smokescreen.

On psychokinesis I think Hardin and Morris have something important to say and I wish I could address myself to their comments. However, the issues involve some nice technicalities and so here I must let my critics have the last word. (I have tried to respond to Hardin and Morris on this matter in private correspondence.)

Hardin and Morris worry about my wanting to experiment on people without their informed consent. They choose to miss the spirit of my remarks. We are not, after all, dealing with risky biomedical experiments but with people who may be claiming to have unusual powers. I am simply suggesting that the best way to ensure against deception is to keep the subjects unfamiliar with the experimental design. I stand by this claim.

Finally, Hardin and Morris make tired old remarks about the lack of decisive experiments. These have been echoed monotonously since the time of Pierre Duhem (circa 1906). The fact is that sometimes there are decisive experiments. Moreover, the fact that Hardin and Morris are interested in the "humble goal" of replicable results has no bearing on the question of whether there are decisive experiments. Those, if they exist, are also replicable. And surely it is better to replicate a good decisive experiment than one that is so awful that its endless replication proves nothing.

Sidney Gendin
Department of Philosophy
Eastern Michigan University
Ypsilanti, Mich.

That which is incapable of proof itself is no proof of anything else.

—Percy Bysshe Shelley (1792-1822)

Past/Present

A new occasional feature, Past/Present will reprint brief articles, exchanges, or commentaries from the past that are of continuing relevance and importance today.—ED.

Anecdote and Coincidence in Parapsychology

Two separate articles in our Winter 1981-82 issue, Ruma Falk's "On Coincidences" and Norman Guttman's review of Louisa Rhine's The Invisible Picture, *dealt with the tendency of many people to attribute more meaning than is justified to coincidence and anecdote in the formation of their beliefs about ESP. Both articles referred to a letter once published on this subject in* Science *by the noted physicist Luis W. Alvarez (who won the Nobel Prize in Physics in 1968 for his discoveries concerning resonance particles). Alvarez's letter led to an interesting exchange between him and the late parapsychologist J. B. Rhine. With the permission of Dr. Alvarez, the estate of J. B. Rhine, and* Science *magazine we here reprint Alvarez's original letter, Rhine's reply, and Alvarez's response to Rhine.*—ED.

A Pseudo Experience in Parapsychology

The popular literature of parapsychology abounds in stories of the following kind: A person is dreaming of a friend he hasn't seen or thought of in 30 years. He is awakened by a call from a telegraph operator telling him of the death of that friend several thousand miles away. Most scientists attribute such pairs of events to pure coincidence, but they offer no comparable stories to show that such apparently improbable coincidences do in fact occur. In the absence of such demonstrations, it is not surprising that the general public feels that there must be a causal relation between the two events, as, for example, by thought transference.

On 16 May 1965, reading the San Francisco *Sunday Chronicle* I noticed an article on page 22 entitled "Ancient Men on the Nile." In scanning this story, I saw the phrase "... the expert appraisal of Philadelphia anthropologist Dr. Carleton S. Coon." To anyone who had been an undergraduate student at the University of Chicago in the early 1930s, this unusual name would call up nostalgic memories of another Carleton Coon and his partner, Joe Sanders, whose popular Coon-

Sanders dance band was then playing at the Blackhawk. So I found myself thinking about Joe Sanders, very probably for the first time in 30 years.

Less than five minues later, having turned the pages of the paper to page 33, I saw an obituary notice headed "Joe Sanders." It read, "... died Friday.... with the late Carleton Coon ... organized the Coon-Sanders band." These two closely spaced recollections of a person forgotten for 30 years, with the second event involving a death notice, is in the classical pattern; but it is obvious that no causal relationship could have existed between the two events.

The probability of a coincidental recollection of a known person in a 5-minute period just before learning of that person's death can easily be calculated, to within a factor of 10. Let us take a 30-year period, and assume that an average person would recognize the names of 3,000 different people who might die in that period of time (3,000 is taken as a geometrical mean of 10^3 and 10^4, the probably extremes of a population of "known persons"). We assume that our subject will learn of the death of each of these persons at some time in the 30 years. If we restrict our attention to the time when our subject learns of the death of a particular person, we can then ask how probable it is that, in the 5 minutes just preceding that exact time of learning of the death, an unrelated recollection that is unique to the 30-year period will occur. This probability, to within a factor of 2, is the ratio of a 5-minute interval to a 30-year interval, or 3×10^{-7}. (It is clear that if one thinks of the particular person once a year rather than once every 30 years, the probability will rise by a factor of 30, to about 10^{-5}.) The probability that one will have such an experience when learning of the death of any one of the 3,000 recognizable persons is clearly 10^{-3} in a 30-year period, or approximately 3×10^{-5} per year. If we take the sample of 10^8 adults in the United States, 3×10^3 experiences of the sort related above should occur per year, or about 10 per day. (For the average person 3,000 recognizable names is probably an overestimate, but the postulated single recollection in 30 years is certainly much too low. These two departures from realistic assumptions have opposite effects on the computed rate, so 10 per day is still a reasonable estimate.) With such a large sample to draw from, it is not surprising that some exceedingly astonishing coincidences are reported in the parapsychological literature as proof of extrasensory perception in one form or another.

LUIS W. ALVAREZ

Physics Department,
University of California, Berkeley

Reprinted from *Science* 148: 1541 (1965)

Parapsychology Not Guilty

Under the heading "A Pseudo Experience in Parapsychology" (Letters, 18 June, p. 1541), Luis W. Alvarez tells of a coincidental experience. He ends his account by saying that such coincidences "are reported in the parapsychological literature as proof of extrasensory perception. ..."

I have long been working with extrasensory perception and with the general

field of parapsychology, and on the basis of my knowledge and experience I can assure Alvarez that no serious worker in this field would ever think of his type of coincidence as in any way parapsychological. His case involved the unusual coincidence of finding two similar names in the same newspaper. Parapsychology deals with a type of communication between person and environment not mediated by the sensorimotor system. It is concerned with coincidence and probability theory only in the same way that any other branch of natural science must be.

There is another aspect to the misunderstandings in the letter. In my 40 years of work in parapsychology I have not come upon any attempt to base a serious conclusion upon spontaneous case material even when such material does belong to the parapsychological category, as that cited by Alvarez does not.

If Alvarez or anyone else wishes to become acquainted with the parapsychological literature to which he refers, a request for a reference list will be promptly filled.

J. B. RHINE

Foundation for Research on the Nature of Man
College Station
Durham, North Carolina

Reprinted from *Science* 149: 910 (1965)

Parapsychology and Spontaneous Cases

J. B. Rhine and Charles G. Morris wrote independent letters ("Parapsychology Not Guilty," 27 Aug., p. 910) in response to my letter entitled "A pseudo experience in parapsychology" (18 June, p. 1541). Morris apparently endorsed my conclusions in his first paragraph, but then, like Rhine, chided me for taking a typical story from the "popular parapsychological literature" rather than from the "more serious parapsychological literature." He concluded that the latter "would be the more appropriate and worthwhile for the inquiring scientist." (In the following quotations, all emphasis has been added by me.)

Rhine concluded his letter with the following two sentences: "*In my 40 years of work in parapsychology I have not come upon any attempt to base a serious conclusion upon spontaneous case material* even when such material does belong to the parapsychological category, as that cited by Alvarez does not. [Rhine apparently missed the point of my example, which was that *no one* could believe that it had parapsychological content.] If Alvarez or anyone else wishes to become acquainted with the parapsychological literature to which he refers, a request for a reference list will be promptly filled."

Naturally I consider myself to be an inquiring scientist, and I do not wish to be unfair to Rhine. So I visited our Psychology Library and took down the latest bound volume of the *International Journal of Parapsychology*. Its table of contents listed a paper entitled "Approaches to the Study of Spontaneous Cases." I confirmed the fact that spontaneous cases are those of the type I used as my model; they are sent to parapsychology laboratories by people who have had unusual

experiences of the kind I tried to show might well be explained as coincidental. One of the references cited in this paper was an article by Louisa E. Rhine, of the Duke University Parapsychology Laboratory, entitled "Conviction and Associated Conditions in Spontaneous Cases" [*J. Parapsychology*. 15, 164 (1951)]. This volume was on the shelf directly below the volume I had just been examining, so it took me only 5 minutes after entering the library to locate Louisa E. Rhine's article.

L. E. Rhine's article is based on a study of 1,600 spontaneous cases on file at Duke. She abstracts 19 of the cases in sufficient detail that one can be convinced she is dealing with examples of the type I referred to in the first paragraph of my note. Her concern is with the degree of conviction felt by the subject that his revelation was authentic and not merely coincidental. The last page of her "Discussion" starts with this paragraph:

> *This case study* has shown first that conviction occurs with considerable frequency in *spontaneous psi experience.* It is reported from widely different sources and, although in varying proportions, in all types of cases. *It is therefore a natural phenomenon* to be studied by appropriate research methods *and should no longer be overlooked by parapsychological experimenters.*

The fact that I was able to discover, in 5 minutes, an example of an *"attempt to base a serious conclusion upon spontaneous case material,"* whereas J. B. Rhine was unsuccessful in 40 years of similar search, may of course be attributable to "luck" on my part.

LUIS W. ALVAREZ

Physics Department,
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There is no error to be named that has not its professors.

—John Locke

From Our Readers

The letters column is a forum for views on matters raised in previous issues. Letters are welcome and are more likely to be published if they are typed and double-spaced. Some may have to be edited.

Sasquatchsickle revisited

The News and Comment item by Eugene Emery on the "Sasquatchsickle" in your Winter 1981-82 issue brought a rush of memories. When the May 1969 *Argosy* hit the stands I was the senior curator of vertebrate paleontology at the Los Angeles County Museum of Natural History. The super sculptor Howard Ball was doing a series of life-sized fiber-glass reconstructions of Rancho la Brea animals for me. The end result was three mammoths, a mastodon, and several sabre-tooth cats. Two giant vultures in the group were vandalized and destroyed within 24 hours of their emplacement at Rancho la Brea—Hancock Park!

The day the magazine came out I made one of my weekly visits to Howard's studio in Torrance. It was a cold day for a Southern California spring and I found Howard and a friend with their feet up to the wood stove drinking beer and having a bad case of hysterics. I went across the street and picked up a six-pack, returned to the studio, pulled up a chair, and popped a lid.

After a decent interval I asked them what the commotion was all about. They would look at me each time I asked, burst into roars of laughter, and point to a copy of *Argosy* on the floor between them.

Finally they calmed down and Howard told me the story. His version was that Floyd Hummiston, like Leonard Bessom, one of my preparators in the lab, had sent an unnamed man to see Howard. Floyd had been in show business for many years, primarily as a lion-tamer. He left that, and after a stint at Disneyland came to me as a preparator. He knew a lot of circus, carny, and show people.

The unnamed man wanted a prehistoric man sculpted in rubber. The two requirements were that it should look as though it had been clubbed and that it should have large genitalia! When Howard had finished his apeman, the person who had ordered it was delighted and carted it away. Howard asked him what he was going to do with it. The man replied, "I've a deepfreeze that I'm going to fill with water, freeze this beast in it, and hit the carny trail."

The *Argosy* article was written by Ivan T. Sanderson, who had done a number of popular natural-history books. I'd always been a bit suspicious of Sanderson's work and when I read the *Argosy* article I went down to see our bookstore manager and suggested that after he had gotten rid of what he had I'd be after his job if he stocked any more of what I considered this man's trash.

J. R. Macdonald
Rapid City, S.D.

I enjoyed your article on the frozen fake

"Bigfoot" displayed by Frank Hansen at fairs. It serves to remind us that the urge to make a buck still produces frauds. I'd also like to point out that nobody who is involved in Sasquatch investigations has ever felt that this frozen dummy was a Bigfoot, ever since Sanderson and Heuvelmans made fools of themselves over this item back in the sixties.

Jon Beckjord, Director
Project Bigfoot
Seattle, Wash.

ESP and logic: One more round

The author of an article has the right to reply to his critics, but if he misunderstands them, and especially if his tone is aggressive, then they too should have the right to reply.

One of the points I tried to make (Letters, Winter 1981-82), in opposition to Gendin's thesis (Summer 1981), is related to his discussion of psychokinesis. If someone can affect the outcome of dice-rolling by thinking, without interaction of any kind known to physics, then by definition he has done it psychokinetically. I don't believe it is possible, but I do not estimate the probability as less than 10^{-100}, whereas Gendin seems to regard the probability as zero. Therefore, sufficiently strong evidence would force me to accept the reality of psychokinesis with or without any further explanation. This would be true even if the method depended largely on putting sugar in coffee, though it seems a somewhat better bet to wish that the die comes up six.

The analogy I intended to give was that wishing to move my arm is more likely to lead indirectly to its motion than is wishing to waggle my left ear. I may have expressed the point badly by using the words "wish" and "will" so that someone trying hard to misunderstand would think I had used them synonymously. The point rather was that I wouldn't will to move my arm unless I wished to move it! I am not really so stupid as to think that "wish" and "will" are synonymous. By jumping on this point, Gendin has missed the intention of my argument. Moreover, I think he will agree that my views about "free will" are not "sophomoric" if he reads my review in the *British J. Philos. Sc. 22* (1971), 382-87, of J. R. Lucas's book *Freedom of the Will.*

The central point where I, *qua* philosopher of statistics, regard Gendin's thesis to be overstated is where he says, "Perhaps we would have a mystery to unravel if a person's wishes correlated highly with success; but, given what we know about the logic of wishing and trying, we have no good reason to postulate that the mystery would point to psychokinesis." It might be very difficult to rule out all forms of known physical forces, and all forms of cheating, but if they could be ruled out then the posterior probability of psychokinesis would, in ratio, greatly exceed its extremely small prior probability. Moreover we would then be inclined to modify what we *think* we know about the logic of wishing and trying. There have been many examples in the history of science when what philosophers thought they knew was overthrown by new discoveries. Readers of the SKEPTICAL INQUIRER will be able to think of examples without difficulty.

I believe that Gendin's argument is essentially formulated when he says: "Even if these experiments are flawless and the statistical analyses are correct, they do not go the slightest way toward establishing that the results were due to ESP, unless we have independent reasons to suppose there is such a thing as ESP." Implicitly this argument requires that the prior probability of ESP is strictly zero. To regard the probability of any empirical possibility as zero would make an acceptable definition of dogmatism. At one time there was no known reason to believe in the moons of Jupiter, so some people

refused to look through Galileo's telescope. Let's not treat the parapsychologists that badly. I don't suppose Gendin will reply by questioning the historical accuracy of the story about Jupiter's moons.

In Gendin's discussion of the prayer experiment that was proposed by J. B. S. Haldane (*not* by me), Gendin assumes that the patients are the people who are supposed to pray. Actually I said that they were to be prayed for. Haldane was an atheist, but he recognized the potential overriding evidential value of experiments.

I. J. Good
Department of Statistics
Virginia Polytechnic Institute
and State University
Blacksburg, Va.

Randi at Cornell

James Randi's appearance before a packed house at Cornell University on January 26, 1982, was a smashing success. Having been a regular reader of the SKEPTICAL INQUIRER for some time, I knew the audience was in for a treat, and no one left disappointed. The girl next to me had come to see Carl Sagan introduce Randi, not really caring who or what a "Randi" was, and at the end she was taking down the address for a subscription to *SI* from the article in the February *Scientific American* that I had brought with me.

Randi's demonstration of "psychic psurgery" (complete with chicken livers and human blood) was a major highlight, and he took questions from the audience for almost an hour before they made us leave.

In that one evening, Randi probably accomplished more for the good of our world than most people do in their lifetime. He should be compelled to regularly visit colleges, whose students are the prime targets of most psychic claptrap, so that the truth can be heard. After all, three days after it reported on Randi's talk, the *Ithaca Journal* ran an article on a local "medium" who has "heard voices talking" since she was small, gives classes in "psychic power," and regularly "leaves her physical body, soaring over houses and trees," all presented at face value. You must never stop fighting this madness.

William D. Edinger
Ithaca, N.Y.

A comment on Schmidt's experiments

In his article (*SI*, Spring 1981), Hansel's Figure 1 shows the results that Schmidt obtained with two subjects. One of them had a steady increase of hits above average and the other had a steady decrease. If there were such a phenomenon as psychokinesis, the subject should be able to decide when he is going to have either a high-score period or a low-score period. He should be able to obtain clear high-score results when he is trying for high scores and clear low-score results when he is trying for low scores, and he should be able to alternate them at will. If the subject is able to get the scores he wants under well-controlled conditions, we would have something in need of accounting. Without such results, I don't think we have anything worth explanation, even if Schmidt improves experiments in a technical way.

Harri Hemilä
Helsinki, Finland

Charlatans confuse public

Bravo for Kendrick Frazier's editorial "Critiques on the Fringe of Science" in the Fall 1981 issue!

A highly vociferous minority of our population considers itself advanced in knowledge and spirituality because its members believe in ghosts, ESP, and a host of born-again miracles.

These people judge themselves holy warriors against unimaginative scientists, humanists, and logical thinkers.

It is the duty of all affirmative persons to denounce the hocus-pocus of intellectually lazy persons who embrace ignorance and superstition. These misologists, haters of learning and discussion, are neither progressive nor visionaries. Rather, they are retrograde persons who want to negate thousands of years of human progress to return to the fears and delusions of our apelike ancestors. Furthermore, these persons who claim to be kidnap victims of Martians, children of virgins, and spokesmen for the gods are obliged to provide extraordinary evidence that they are not liars, lunatics, or ignoramuses. The burden of proof is clearly theirs.

Many charlatans are confusing the public with the argument that, since scientists disagree on some issues, all truth is ill-defined and up for grabs. This extreme form of skepticism, nihilism, erodes the foundation of all rational wisdom and communication. This irrationalism is the stuff of which totalitarian regimes are born again.

André Bacard
Modern Studies Group
Stanford, Calif.

Mysterious world

In reviewing Welfare and Fairley's *Arthur C. Clarke's Mysterious World,* J. Richard Greenwell states that "Loch Ness and dozens of other lakes and fjords of western Scotland were carved out by glacial displacements and *subsequently* became lakes and fjords . . ." (*SI,* Summer 1981).

Loch Ness (and lochs Lochy, Oich, and Linnhe) fill a depression formed by a major crustal fault that existed well before the ice ages, although it was enlarged by glaciation. It is true that other "monster" lochs, such as Morar, were cut by glaciers. (We do not have "fjords" in Scotland.)

The question "Could it [Nessie] have survived the freezing over of the lake in the last ice age?" is still inappropriate, and ignores the possibility that "monsters" could have entered the lake *after* the last ice age, when it was still an arm of the sea.

Chapter 12 of the Welfare and Fairley book contains the Jennings "ball lightning" photograph, which I have shown was caused by a street lamp ("How not to photograph ball lightning" in *British Journal of Photography,* Oct. 23, 1981, pp. 1096-97, 1105), and a picture of the McIntosh hole in window glass, which I have shown was not caused by ball lightning ("Not Lightning Damage" in *Weather,* March 1981, pp. 66-71). The ball-lightning event at Crail (Fife) in 1968 (not 1966 as stated) will be described in a forthcoming special issue of *Weather.*

Steuart Campbell
Edinburgh, Scotland

Paranoia of nonchance reality

I find the SKEPTICAL INQUIRER an invaluable journal. As a layman with a questioning mind, I groped the frustrating path from gullibility to skeptic with little knowledge of how to proceed. Your journal has helped substantially.

In his review of Louisa Rhine's book (Winter 1981-82), Norman Guttman stated something that particularly struck me. He points out that Rhine's kind of thinking (eliminating chance as a mode of explanation), if commonly adopted, would nullify and undermine one of our basic ways of understanding the world.

The "human potential movement," a current national phenomenon with California origins, promotes exactly that kind of thinking. I have participated in several such groups and have encountered many within them who, with the organization's blessings, eschew the notion of chance.

The gospel according to the groups I'm familiar with is that we each "create" our realities, and that to attribute anything to chance is a cop-out. This is useful to a degree, but can easily lead to what Guttman terms very precisely as a "certain special kind of paranoia."

I have seen numerous occasions where that paranoia became emotionally damaging: a woman relentlessly searching her psyche to fathom why she had "created" her abdominal cyst; another who was very distressed by an auto accident ahead of her on the freeway that she perceived she had "created" as a convenient excuse to avoid arriving on time somewhere. I could go on and on.

I have also met several individuals within these "consciousness" groups who casually reject the notion of an "objective reality." They hold to a sophomoric solipsism from the security of a comfortable material existence.

The SKEPTICAL INQUIRER has been a mainstay of my ability to deal with all this modern hokum. Given the reality that bunk can be so easy, so much fun, and so profitable, and debunking so thankless, time-consuming, and profitless, I would like to thank the SKEPTICAL INQUIRER and its contributors for your efforts. I just wish that the habit of critical thinking was more contagious.

Jay Hendon
Palo Alto, Calif.

Bio-error

In my eyes, the following is another good criticism of the biorhythm theory (*SI*, Summer 1979). Even if there existed such things as a 23-day physical cycle, a 28-day emotional cycle, and a 33-day intellectual cycle, how could we seriously believe that the first cycle had a length of *exactly* 23 days, *zero hours and zero minutes*, the second exactly 28 days, and the third exactly 33 days?

Who discovered these periods? If they were obtained by statistical studies, then what is their probable errors? For the first cycle, for instance, we normally would expect a result like 23.02 ± 0.05 days.

If the exact length of the cycle is really 23.01 days instead of exactly 23, then the error will amount to one full day after 100 cycles, that is 6.3 years. Then, for an adult aged 38 years, the error would be 6 days, which renders all that biorhythmic speculation obsolete.

Jean Meeus
Erps-Kwerps, Belgium

Some comfort

It may or may not be of some comfort to note that we're hardly the first generation to be plagued with pseudoscience, as witnessed eloquently by the sixteenth-century historian, Guicciardini: "How happy are the astrologers, who are believed if they tell one truth to a hundred lies, while other people lose all credit if they tell one lie to a hundred truths."

N. C. Jones
Winchester, Va

Starchild

Recently my local newspaper, the *Seattle Post-Intelligencer*, prominently displayed an article about proponents of astrological birth-control. The article failed to mention what users of astrological birth control are called. Usually they are called parents.

Emily Sisson
Seattle, Wash.

The Committee for the Scientific Investigation of Claims of the Paranormal

Paul Kurtz, Chairman

Scientific and Technical Consultants

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The Committee for the Scientific Investigation of Claims of the Paranormal

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